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DIVERSIFYING SOCIETY'S LEADERS? THE DETERMINANTS AND CAUSAL EFFECTS
OF ADMISSION TO HIGHLY SELECTIVE PRIVATE COLLEGES

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Diversifying Society's Leaders? The Determinants and Causal Effects of Admission to Highly Selective Private Colleges

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ABSTRACT

We use anonymized admissions data from several colleges linked to income tax records and SAT and ACT test scores to study the determinants and causal effects of attending Ivy-Plus colleges (Ivy League, Stanford, MIT, Duke, and Chicago). Children from families in the top 1% are more than twice as likely to attend an Ivy-Plus college as those from middle-class families with comparable SAT/ACT scores. Two-thirds of this gap is due to higher admissions rates for students with comparable test scores from high-income families; the remaining third is due to differences in rates of application and matriculation. In contrast, children from high-income families have no admissions advantage at flagship public colleges. The high-income admissions advantage at Ivy-Plus colleges is driven by three factors: (1) preferences for children of alumni, (2) weight placed on non-academic credentials, and (3) athletic recruitment. Using a new research design that isolates idiosyncratic variation in admissions decisions for waitlisted applicants, we show that attending an Ivy-Plus college instead of the average flagship public college increases students' chances of reaching the top 1% of the earnings distribution by 50%, nearly doubles their chances of attending an elite graduate school, and almost triples their chances of working at a prestigious firm. The three factors that give children from high-income families an admissions advantage are uncorrelated or negatively correlated with post-college outcomes, whereas academic credentials such as SAT/ACT scores are highly predictive of post-college success.

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1 Introduction

Leadership positions in the United States are held disproportionately by graduates of a small number of highly selective private colleges. Less than half of one percent of Americans attend Ivy-Plus colleges (the eight Ivy League colleges, Chicago, Duke, MIT, and Stanford). Yet these twelve colleges account for more than 10% of Fortune 500 CEOs, a quarter of U.S. senators, and three-fourths of Supreme Court justices appointed in the last half-century (Figure 1). Ivy-Plus colleges also enroll a disproportionate share of students from high-income families: students from families in the top 1% of the income distribution are more than twice as likely to attend an Ivy-Plus college than students with comparable SAT or ACT scores from the middle class (Figure 2).

These facts motivate our central questions: Does attending a highly selective private college open pathways to high-status, high-paying positions that students would not otherwise have had? If so, could such colleges increase the fraction of society’s leaders who come from low- and middle-income families by changing their admissions policies?

The answers to these questions depend on two sub-questions, the first related to the inputs into colleges and the second related to their outputs. First, how much of the disproportionate representation of students from high-income families at highly selective private colleges is driven by preferential admissions practices vs. student choices about where to apply and matriculate? Second, do such colleges have a causal effect on students’ post-college outcomes, or would the students they admit have done equally well if they had attended other colleges?

We study these questions using a new anonymized panel dataset that links several sources of administrative data: (1) information from parents’ and students’ federal income tax records; (2) college attendance information from the Department of Education; (3) data from the College Board and ACT on standardized test scores; and (4) application and admissions records from several highly selective public and private colleges covering 2.4 million students. This dataset provides longitudinal information on a rich set of pre-college characteristics (parental income, students’ SAT and ACT scores, high school grades, academic and non-academic credentials) as well as early career post-college outcomes (earnings, employers, occupations, graduate school attendance). Within this dataset, we focus on the 12 Ivy-Plus colleges, 12 other highly selective private colleges (e.g., Northwestern University and Washington University), and 9 flagship public institutions (e.g., University of California Berkeley and University of Michigan Ann Arbor). We focus on the entering classes of 2010-15 when analyzing attendance patterns and include earlier cohorts to analyze long-term post-college outcomes.

We divide our analysis into four parts. We begin by examining why children from high-income families are more likely to attend Ivy-Plus colleges by analyzing the pipeline to college enrollment, from application to admission to matriculation (enrollment conditional on admission). Conditional on pre-college academic qualifications – as measured by SAT and ACT scores – students from high-income families apply to highly

selective private colleges at slightly higher rates than those from lower-income families.¹ These differences in application rates explain 20% of the income gap in attendance conditional on SAT/ACT scores.²

Two-thirds of the difference in enrollment rates at Ivy-Plus colleges by parental income can be explained by higher *admissions* rates for students from high-income families. Conditional on SAT/ACT scores, applicants from families in the top 1% (incomes > \$611,000) are 58% more likely to be admitted to Ivy-Plus colleges than applicants from middle-class families, which we define in this study as those with parental incomes between the 70th and 80th percentiles of the national income distribution (\$91,000-\$114,000), roughly the middle decile of the parental income distribution for applicants to highly selective colleges. Conditional on admission, children from high-income families are slightly more likely to enroll (matriculate) at an Ivy-Plus college, explaining the remaining 12% of the gap in attendance rates.

The admissions advantage for students from top 1% families arises from three factors. 24% of the admissions advantage is explained by the recruitment of athletes, who tend to come from higher-income families. Another 46% is driven by preferential admission for students whose parents attended the same college (“legacies”). Legacy students from families in the top 1% are five times as likely to be admitted as the average applicant with similar test scores, demographic characteristics, and admissions office ratings. Children of alumni of a given Ivy-Plus college have no higher chance of being admitted to other Ivy-Plus colleges, indicating that legacy status does not simply proxy for other unobservable credentials that lead to higher admissions rates. The remaining 31% of the admissions advantage for students from families in the top 1% is explained by their stronger *non-academic* credentials (e.g., extracurricular activities, leadership traits, etc.). Children from the top 1% are much more likely to attend private high schools, whose students have much higher non-academic ratings (but no higher academic ratings) than children from public high schools with comparable SAT/ACT scores. These three factors are unique to the admissions processes of *private* colleges. At flagship public institutions, admissions rates are uncorrelated with parent income conditional on SAT/ACT scores.³

In light of these findings about differences in admissions rates, the second part of the paper analyzes whether admitting more low- and middle-income students to Ivy-Plus colleges would increase their chances of early career success after college and ultimately diversify society’s leaders. We cannot directly estimate treatment effects on the leadership outcomes considered in Figure 1 because we only observe students’ outcomes until their early thirties and very few people reach leadership positions by that point. We address

¹Throughout this paper, we use SAT/ACT scores as a baseline measure of pre-college academic qualifications. Standardized test scores may not be pure measures of academic “merit” insofar as children from high-income families may have access to additional test preparation or other resources that allow them to obtain higher scores (e.g., Goodman, Gurantz, and Smith 2020). Such factors would only lead us to underestimate the disparities in college attendance by parental income conditional on academic merit, amplifying the arguments made below.

²Our findings differ from those of Hoxby and Avery (2013), who identify differences in application rates as a key factor that explains why selective private colleges have fewer low-income students, because we measure parental income at the individual level rather than using geographic imputations (see Chetty et al., 2020) and because we study a more recent time period, after private colleges and non-profits had expanded programs to recruit applicants from lower-income backgrounds. Our findings are consistent with those of Dynarski et al. (2021), who focus on public colleges, in that we too find significant gradients in application rates by parental income at many public institutions.

³However, application rates differ substantially by parental income at public institutions, leading to large gaps in attendance rates. For example, children from the top 1% are 62% more likely to apply to flagship public universities than children with parents between the 70th and 80th percentiles of the national income distribution.

this problem using early career indicators of success – reaching the top 1% of the income distribution, attending an elite graduate school, and working at a prestigious firm – that we show are strong predictors of subsequently achieving leadership outcomes. We directly estimate the impact of Ivy-Plus attendance on early career success – which are of interest in their own right – and then predict impacts on leadership outcomes under additional assumptions described below.

We estimate the causal effect of attending an Ivy-Plus college instead of the average flagship public university on post-college outcomes using two research designs. Our first research design isolates idiosyncratic noise in admissions decisions that is plausibly orthogonal to candidates' potential outcomes by exploiting the fact that we observe admissions decisions for a given candidate at several colleges. We consider a statistical model in which admissions decisions are a function of (1) students' latent abilities, defined as factors that are correlated with long-term outcomes and (2) other idiosyncratic factors that matter for admissions at a particular college but do not affect long-term outcomes (e.g., whether the student plays a musical instrument that is needed to fill a college orchestra). We do not directly observe such idiosyncratic factors, but isolate variation arising from them in two steps. We first focus on the subset of applicants who are waitlisted at a given college and are thus on the margin for admission. We then test whether applicants who are admitted vs. rejected from the waitlist at a given Ivy-Plus college have comparable latent abilities by examining whether their chances of admission to *other* Ivy-Plus colleges differ. Intuitively, if colleges with similar admissions practices make uncorrelated admissions decisions, then the residual variation in admissions conditional on being on the waitlist must be due to idiosyncratic factors uncorrelated with students' long-term potential outcomes.

Implementing this test using data from several Ivy-Plus colleges, we find that admissions outcomes among waitlisted applicants at any given Ivy-Plus college are uncorrelated with the admissions decisions and internal ratings of other Ivy-Plus colleges. We show that under the identification assumption that different college admission committees' assessments of a candidate's underlying merit (i.e., the component that predicts long-term outcomes) are positively correlated with each other, comparisons of students who are admitted vs. rejected from the waitlist can be used to identify the causal effect of admission for marginal applicants.

Using this design, we find that being admitted from the waitlist to an Ivy-Plus college increases students' chances of achieving early career upper-tail success on both monetary and non-monetary dimensions. The causal effects of admission to an Ivy-Plus college are much larger for students with weaker fallback options – e.g., whose colleges in their home state channel fewer students to the top 1% after college. Exploiting this heterogeneity in treatment effects, we estimate that the marginal student who is admitted to and attends an Ivy-Plus college instead of the average flagship public is about 50% more likely to reach the top 1% of the income distribution at age 33, nearly twice as likely to attend a highly-ranked graduate school, and 2.5 times as likely to work at a prestigious firm. Attending an Ivy-Plus college has an especially large effect on students' chances of reaching the upper quantiles of the income distribution. The impact of Ivy-Plus admission on reaching the top quartile of the distribution is small and statistically insignificant, while the

impact on chances of reaching the top 1% far exceed what one would predict based on a constant percentage treatment effect across the income distribution. As a result of these upper-tail impacts, attending an Ivy-Plus college increases mean earnings by \$101,000 at age 33 (relative to a counterfactual mean of \$143,000 if the same students were to attend state flagships).

These findings differ from a well-known set of studies which conclude that attending a highly selective college in the U.S. has little impact on students' earnings (Dale and Krueger 2002, Dale and Krueger 2014, Mountjoy and Hickman 2021, Ge, Isaac, and Miller 2022). To investigate why our conclusions differ, we replicate the research design used in those studies by comparing earnings outcomes for students who attend different colleges, controlling for the set of colleges to which they were admitted. This matriculation design yields estimates very similar to and statistically indistinguishable from those obtained from our waitlist research design: students who choose to attend Ivy-Plus colleges instead of state flagship colleges (conditional on being admitted to both) are significantly more likely to reach the top 1% of the income distribution, attend an elite graduate school, and work at prestigious firms. However, the matriculation design again implies modest impacts of attending an Ivy-Plus college on log earnings, consistent with the findings of Dale and Krueger (2002), whose primary outcome is log earnings. Furthermore, we find little association between students' average outcomes and the mean test scores of the college they choose to attend, the proxy for college quality used by Dale and Krueger and others. In short, our findings differ from the conclusions of prior studies not because of differences in research design but rather because our richer data allow us to directly identify college's fixed effects (rather than using proxies for quality such as test scores) and isolate impacts on upper tail outcomes, where Ivy-Plus colleges have the largest impacts.

In the third part of the paper, we analyze whether the credentials underlying the high-income admissions advantage (legacy, athlete status, high non-academic ratings) are predictive of better post-college outcomes. We find that recruited athletes, students with higher non-academic ratings, and legacy students have equivalent or *lower* chances of reaching the upper tail of the income distribution, attending an elite graduate school, or working at a prestigious firm than comparable Ivy-Plus applicants once we adjust for the fact that they are admitted to better colleges. By contrast, academic ratings and SAT/ACT scores are highly predictive of post-college outcomes: students with higher standardized test scores have significantly higher levels of earnings, are more likely to attend top graduate schools, and work at prestigious firms after college.

In the last part of the paper, we combine the estimates from our pipeline and causal effects analyses to answer our motivating question: how much could Ivy-Plus colleges diversify their student bodies and society's leaders by changing their admissions practices? We consider a counterfactual admissions scenario in which colleges eliminate the three factors that drive the admissions advantage for students from high-income families – legacy preferences, the weight placed on non-academic ratings, and the differential recruitment of athletes from high-income families – and refill slots with students who have the same distribution of test scores as the current class. Such an admissions policy would increase the share of students attending Ivy-Plus colleges from the bottom 95% of the parental income distribution by 8.8 percentage points (pp),

comparable to the effects of race-based affirmative action policies on the share of Black and Hispanic students (Card 2017). Alternatively, one can generate comparable increases in socioeconomic diversity while retaining current admissions preferences by giving admissions “boosts” for highly qualified low-income students that are about 1/3 as large as those currently given to legacy applicants. Importantly, the increases in socioeconomic diversity from such policies would not come at the cost of reducing class quality, as judged by post-college early career outcomes, because the factors leading to admissions advantages for students from high-income families are not predictors of better outcomes as discussed above.

We predict how these counterfactual admissions policies would affect the socioeconomic backgrounds of society’s leaders by extrapolating from the estimated impacts on early-career outcomes. The key assumption underlying our extrapolation is that the share of the observational difference in rates of achieving leadership outcomes (e.g., becoming a Supreme court justice) between students at Ivy-Plus and flagship public colleges that is due to the causal effect of Ivy-Plus attendance is the same as the causal share of observational differences in early-career outcomes (e.g., working at prestigious firm). We predict that changes in Ivy-Plus admissions policies would have small effects on the backgrounds of individuals achieving upper-tail monetary outcomes (e.g., reaching the top 1% or becoming a CEO) simply because Ivy-Plus attendees account for a relatively small share of individuals who reach the top of the income distribution; eliminating the three high-income admissions advantages would increase the share of Fortune 500 CEOs from families with parental income below the 95th percentile by 0.4pp.

Admissions changes would have larger effects for non-monetary outcomes – for instance, increasing the share of senators from families with income in the bottom 95% by 1.7pp and Supreme Court justices by 5.4pp – since a much larger fraction of individuals in these positions attend Ivy-Plus institutions (as shown in Figure 1). The effects would be even larger with need-affirmative admissions policies that go beyond an income-neutral benchmark and give preferences commensurate to those currently given to legacy applicants to students from lower-income backgrounds, which we predict would increase the share of Supreme court justices coming from families in the bottom 60% by 17.5 pp. These predictions must be interpreted as rough estimates because we can only measure early-career success rather than leadership outcomes directly and because our calculations ignore general equilibrium effects, such as changes in colleges’ causal effects as a result of changes in the composition of their student bodies (as in, e.g., Carrell, Sacerdote, and West 2013). Nevertheless, the estimates suggest that a small number of highly selective private colleges could measurably change the socioeconomic backgrounds of individuals who hold influential positions in society by changing their admissions policies.

This study builds on an extensive literature studying the determinants and consequences of admission to elite colleges. The literature on determinants of admission has focused primarily on racial disparities; data on socioeconomic diversity, particularly in the very upper tail of the income distribution, have been much more scarce (e.g., Bowen and Bok 2000). We show that our findings regarding socioeconomic diversity hold conditional on race, but we do not study the role of race in admissions directly because it has been examined

extensively in other recent work (e.g., Espenshade, Chung, and Walling 2004; Card 2017; Arcidiacono, Kinsler, and Ransom 2022). The literature on the consequences of higher education has likewise been hampered by an inability to follow large numbers of students over time after college, particularly at elite private colleges. While several studies have documented large causal effects of attending selective public colleges using admissions thresholds to implement regression discontinuity designs in administrative data (e.g., Hoekstra 2009; Zimmerman 2014; Bleemer 2021b; Kozakowski 2023), private colleges in the U.S. do not use such admissions thresholds. Using admissions data from multiple private colleges, we formulate new research designs that allow us to identify the causal effects of attending private colleges, showing that they have particularly large effects on upper tail outcomes, consistent with the findings of Zimmerman (2019) in Chile.

The paper is organized as follows. The next section describes the data we use. Section 3 characterizes the pipeline to college enrollment by parental income. Section 4 presents evidence on the causal effects of attending Ivy-Plus colleges. Section 5 examines how post-college outcomes vary with students' application credentials. Section 6 analyzes the impacts of changes in admissions practices. Section 7 concludes.

2 Data

We construct a de-identified dataset on parent characteristics and student outcomes by linking five sources of data: (1) federal income tax records on parents and children's incomes from 1996-2021; (2) 1098-T tax forms on college attendance from 1999-2015; (3) Pell grant records from the Department of Education's National Student Loan Data System from 1999-2013; (4) standardized test score data from the College Board from 2001-2005 and every other year from 2007-15 and ACT from 2001-15; and (5) applications and admissions records for undergraduate first-year student admissions spanning subsets of years from 1998-2015 from several Ivy-Plus colleges and flagship public universities, as well as data for all colleges in the University of California (UC) and California State University (CSU) systems and all four-year public colleges in Texas from the Texas Higher Education Coordinating Board (THECB). We include data from UC-Berkeley, UCLA, and UT-Austin among others in our sample of flagship public universities with internal data; unfortunately our agreements with Ivy-Plus colleges do not permit us to name the specific partners or the total number of colleges (in order to preclude re-identification based on total sample counts). These five sets of data were linked to each other at the individual level by social security number and/or identifying information such as name, date of birth, and gender.⁴ All analyses were then conducted using the linked individual-level dataset after it was stripped of personally identifiable information.

In this section, we describe our analysis samples, define the key variables we use, and present summary statistics.

⁴Within our target sample of U.S. citizens or permanent residents, we link more than 90% of the individuals who appear in datasets 2-5 to the income tax records.

2.1 Sample Definitions

Our target analysis sample is college applicants who are U.S. citizens or permanent residents with parents in the U.S. who took either the SAT or ACT. We focus on U.S. citizens and permanent residents with parents in the U.S. because those are the students for whom we observe parental income; we have no data on parental incomes for international students. Virtually all students who apply to highly selective colleges take either the SAT or ACT over the period we study; we restrict attention to college applicants who took one of these tests because we use those scores as baseline measures of pre-college academic preparation.

Due to differences in data availability across colleges, we use three different samples in our analysis, defined below.

Pipeline Analysis Sample. When characterizing the pipeline to college enrollment by college (as in Section 3), we construct our analysis sample by starting from the raw income tax data (described in Appendix A of Chetty et al. 2020) and retaining the subset of individuals who (1) have a valid Social Security Number (SSN) or Individual Taxpayer Identification Number (ITIN), (2) can be linked to parents, and (3) appear in either the SAT or ACT data in 2011, 2013, or 2015.⁵ We define each child’s “parent” as the person who most recently claimed the child as a dependent between child ages 12–17. If the child is claimed by a single filer, the child is defined as having a single parent; if the child is claimed by joint filers, both filers are defined as parents. Children who are not claimed as dependents on any tax return are not linked to parents and are excluded from our analysis.⁶

College-Specific Analysis Sample. When studying admissions and matriculation at specific colleges (Section 3.2), admissions decisions (Section 3.3), and the causal effects of colleges on outcomes (Section 4), we focus on the subset of Ivy-Plus and flagship public colleges for which we have internal application and admissions data. In these analyses, we define the analysis sample as all permanent residents or citizens in the college-specific dataset who submitted a first-year undergraduate application to the college over the years for which we have data who (1) can be linked to the tax data based on their SSNs or ITINs and (2) can be linked to parents in the tax data.⁷

Long-Term Outcomes Sample. Because the data we have from most colleges are for relatively recent cohorts, we observe earnings when individuals are in their thirties for a relatively small sample. We address this limitation by building prediction models for later earnings based on individuals’ employers at ages 22–25, which we observe for many more students given our cohort restrictions. We estimate these models using data from the 1977–88 birth cohorts, including all individuals with valid SSNs or ITINs in the tax data (irrespective of whether they can be linked to parents).

⁵The first two restrictions are intended to isolate citizens and permanent residents of the U.S. (as we do not observe citizenship status in the tax records). The SAT and ACT data are organized by the year in which students would graduate from high school if they graduate in four years; for the vast majority of students, this corresponds to the year in which they apply to college. We focus on 2011, 2013, and 2015 because SAT data are available only in odd years.

⁶Because almost all U.S. residents file at least one tax return in a year when their child is between ages 12 and 17, we are able to link more than 98 percent of children born in the U.S. to parents (Chetty et al. 2020, Appendix Table I).

⁷We also exclude a small number of applicants who are born after 1996 or are older than 21 in the year they would enter the college to ensure that individuals have had adequate time to complete a four-year degree when we measure post-college outcomes at age 25.

2.2 College Application, Admission, and Attendance

College Attendance. We measure college attendance for children in our samples using two methods. First, following Chetty et al. (2020), we combine data from tax records (1098-T forms) and the National Student Loan Data System (Pell Grant records) to obtain a roster of college attendance covering all colleges in the U.S., as described in Appendix B. Second, we use colleges' own admissions records, defining attendance using an indicator for whether an admitted student matriculated to a given university as a first-year undergraduate student.

Each of these measures of attendance has certain advantages. The measures based on federal administrative data cover all colleges but are imperfect in that they sometimes do not distinguish between specific campuses of multi-campus state universities or distinguish summer school students from regular full-time undergraduates. Attendance measures based on colleges' own datasets are more accurate, but are available only for the subset of colleges and years for which we have admissions data. When both attendance measures are available, they are typically well aligned; however, when they differ, we use college-specific data to measure attendance where available. Note that we do not observe degree completion in either dataset, so students are assigned to colleges based on attendance without regard to graduation.

We focus on three groups of colleges in our primary analysis: (1) Ivy-Plus colleges, which includes the Ivy League, Stanford, Duke, MIT, and Chicago (12 colleges); (2) other highly selective private colleges (the 12 highest ranked private colleges according to the 2022-2023 U.S. News and World Report for National Universities, excluding the Ivy-Plus); and (3) 9 flagship public colleges for which we have data. These colleges are listed in Appendix Table 1.

College Application and Admissions. For the college-specific analysis sample, we observe additional information from colleges' application and admissions records: demographic information (self-reported race and ethnicity and gender), indicators for being a legacy (one or more of the child's parents obtained an undergraduate degree from the college to which the student applied) or child of a faculty member (one of the parents is currently a tenure-track faculty member at the college) status, high school grade point averages, and a flag for whether the student was a recruited athlete. For Ivy-Plus colleges, we additionally observe information on admissions office ratings of applicants. While the exact set and scaling of ratings differ by college, the ratings are integer-valued and typically measure academic and non-academic aspects of an application separately. We also observe information on whether students applied in the "Early" or "Regular" application cycles and whether they were placed on a waitlist after the regular admissions cycle.⁸

Standardized Test Scores. We obtain data on standardized test scores from the College Board and ACT. We focus on a student's composite SAT score, defined as the mathematics score plus the critical reading score, and the composite ACT score (ranging from 1 to 36). We map ACT scores into equivalent SAT scores using published concordance tables (ACT, 2016), prioritizing SAT scores when both scores are available. We

⁸During the period we study, early applicants applied in the late fall and received an admissions decision by December. Regular applicants submitted applications in the winter and received decisions in the Spring. Some students not admitted in either of the two rounds were placed on a waitlist. After the two rounds of admissions decisions and student matriculation decisions, colleges offered available slots to certain students on the waitlist.

use the subset of years for which we have both SAT and ACT data: 2001-2005 and odd years from 2007-15. We use students' most recent test scores if they have taken a test multiple times.⁹

The College Board and ACT report student test scores to colleges at students' request. Since sending one's score to a college indicates an intention to apply to that college, we use this score-send data to construct a prediction model for application to colleges for which we do not have internal applications records in our pipeline analysis (see Appendix C).

2.3 Parental Income and Post-College Outcomes

We obtain data on children's and parents' incomes from income tax returns (1040 forms) and third-party information returns (e.g., W-2 forms), which contain information on the earnings of those who do not file tax returns. We measure income in 2015 dollars, adjusting for inflation using the consumer price index (CPI-U).

Parental Income. Our primary measure of parental income is total household-level pre-tax income. In years in which a child's parent files an income tax return, we define household income as the Adjusted Gross Income reported on the 1040 tax return. In years in which a parent does not file an income tax return, we define household income as the sum of wage earnings (reported on form W-2) and unemployment benefits (reported on form 1099-G) for all parents linked to a child. In years in which parents neither file tax returns nor receive information returns, household income is coded as zero. Chetty et al. (2020) show that these income definitions yield an income distribution similar to that in the American Community Survey (ACS) under the same income definitions.

We average parents' household income over the years in which their child is between 12 and 17 years old to smooth year-to-year income fluctuations and estimate the resources available to a household when a child chooses to attend college.¹⁰ We then assign parents income ranks relative to all other parents with children in the same birth cohort.

Child Income. We define children's incomes in adulthood as total pre-tax *individual* income. For single filers, we define individual income as the sum of wage earnings and net self-employment income if positive as reported on 1040 tax returns. For those who file jointly, we define individual income as the sum of (1) individual wage earnings reported on W-2 forms; (2) individual net self-employment income (if positive) reported on Form SE; and (3) the difference between total wage earnings and self-employment income reported on Form 1040 and the sum of both filers' W-2 wage earnings and form SE income. For non-filers, we define income as the sum of wage earnings reported on W-2 forms; if an individual does not receive W-2 forms, we report income as zero. We then rank children based on this income measure relative to the national distribution for their birth cohort.

⁹The college-specific datasets also contain data on students' standardized test scores as part of their applications. In the college-specific analysis sample, we prioritize the test score reported in student applications (and analyze all available years of data); if that score is missing, we use data from the College Board and ACT files. For colleges in the University of California system, we always prioritize the scores reported by the College Board and ACT because the SAT scores in the UC system's internal data include scores on a separate writing section that was not administered systematically in all years of our sample.

¹⁰We limit the sample to parents with non-negative income because those with negative income typically have large business losses, which are a proxy for having significant wealth. The non-negative income restriction excludes less than 1% of children from our sample.

Graduate Schools. We use data from 1098-T forms to measure graduate school attendance at various ages. The 1098-T forms include a flag for graduate school attendance, but they do not include information on the type of graduate school attended (e.g., medicine, law, business, etc.). In our baseline analysis, we define “elite” graduate schools as Ivy-Plus institutions, as well as UC-Berkeley, UCLA, UCSF, University of Michigan, and University of Virginia; we evaluate robustness to alternative definitions below.

Predicted Incomes Based on Early-Career Graduate Schools and Employers. Because income ranks do not stabilize until students are in their early thirties, we use data on individuals’ employers and graduate schools to predict incomes at age 33. We first measure individuals’ graduate school attendance and employment (based on 1098-T and W-2 forms, respectively) in the years in which they turned 22-25. We assign individuals a graduate school or employer at each age based first on the graduate school they attend and, if they are not in graduate school, based on the employer from which they received the most W-2 earnings. Those who receive neither a 1098-T nor a W-2 in a given year are pooled into a separate “not classified” category. We then regress our key outcomes at age 33 – the probability of having earnings in the top 1% of the birth cohort and mean income rank – on graduate school and employer fixed effects interacted with age (22-25) fixed effects and construct predicted values from these regressions – a surrogate index in the terminology of Athey et al. (2019). See Appendix D for further details.

Elite and Prestigious Employers. We construct measures of “elite” and “prestigious” employers that expand upon conventional lists of high-status jobs based on the revealed preferences of Ivy-Plus graduates. In particular, we define elite firms as those that disproportionately employ students from Ivy-Plus colleges. We first calculate the share of all Ivy-Plus attendees in the 1979 to 1996 birth cohorts that work at each firm when they are age 25. We then calculate the same share for the flagship public colleges, and compute a ratio of those shares, restricting the sample to firms that employ at least 25 college attendees from the 1979-96 birth cohorts and excluding each individual’s own college from the ratio.¹¹ In our baseline analysis, we rank firms using this metric and define a firm as “elite” by pulling firms from the top of the list until we have accounted for 25% of Ivy-Plus attendee employment (see Appendix E for further details); we evaluate robustness to alternative definitions below.

Many of the elite firms by this definition also have high predicted income ranks. To measure high-status jobs that do not necessarily lead to high earnings, we regress the ratio of the shares defined above on a quintic function of the firm’s predicted top 1% probability defined above. We then calculate the residual from this regression and re-rank firms accordingly. We call the top firms which account for 25% of Ivy-Plus employment “prestigious” employers. Intuitively, this outcome measures firms that disproportionately employ non-sample Ivy-Plus attendees conditional on their salaries.

We directly validate this algorithmic approach to identifying elite and prestigious employers by comparing the firms identified by our algorithm to publicly available rankings of firms in various industries. We find a

¹¹We use flagship public colleges as a comparison group to exclude individuals who work in lower-status occupations within elite firms (e.g., as administrative assistants); empirically, we find that graduates of flagship public colleges rarely take such positions. However, expanding the comparison colleges to include all selective four-year colleges (Tiers 2-4 as classified by Barron’s rankings) yields very similar results (see Appendix Figure 33 below).

high degree of overlap. Among the 10 largest law firms that we identify as “prestigious,” 5 are also ranked among the top 10 most prestigious law firms by an external (Vault.com) ranking. Similarly, 4 of the 5 largest consulting firms we identify as “prestigious” are among the top 5 most prestigious as well according to the same (Vault.com) ranking. Of the 10 largest prestigious hospitals by our definition, 5 are ranked among the 10 top hospitals that treat patients (by the institutional research ranking site Scimagoir.com). 7 of the 10 largest prestigious universities we identify are Ivy-Plus institutions.¹²

Children’s Demographics. We obtain information on children’s sex (from 1993 onward) and year of birth from the Death Master (also known as the Data Master-1) file produced by the Social Security Administration and housed alongside tax records. We obtain information on children’s self-reported race and ethnicity from the College Board and ACT datasets. For the college-specific analysis sample, we prioritize information on applicants’ race and ethnicity as reported by students in their college applications.

2.4 Summary Statistics

Table 1 presents descriptive statistics for the three analysis samples defined in Section 2.1.

Column 1 lists summary statistics for the pipeline analysis sample. While 93% of the 5.1 million SAT/ACT takers in this sample attended a college at some point between the ages of 19 and 22, only a small share attended one of the highly selective colleges we focus on in this study (e.g., 0.8% at Ivy-Plus colleges). Column 2 lists summary statistics for our long-term outcomes sample, subset to those who took the SAT or ACT for comparability to the pipeline analysis sample. Although older than the pipeline sample, these individuals are very similar in terms of the distribution of colleges they attend and demographic characteristics.

Columns 3 and 4 present summary statistics for our college-specific samples from the Ivy-Plus and flagship public colleges for which we have internal applications and admissions records. As expected, students who apply to these selective colleges have considerably higher test scores than students in the broader pipeline analysis sample that includes all test takers (e.g., 1374 for applicants to Ivy-Plus colleges compared with 991 for test takers overall). The mean parental household income ranks of children in these samples who applied to Ivy-Plus and flagship public colleges are also higher than in the general population, at 72.3 and 78.0, respectively. We therefore define individuals with parental income between the 70th and 80th percentile of the national parental income distribution as the “middle class” for the purposes of our analysis, since we focus on applicants to highly selective colleges. Post-college outcomes are also better for the Ivy-Plus and state flagship applicant samples relative to the broader long-run outcomes sample.

The preceding statistics pertain to students who *applied* to different types of colleges. In Appendix Table 2, we present analogous summary statistics for the pipeline, long-term outcomes, and college-specific samples for individuals who *attended* Ivy-Plus, flagship public, or other selective private colleges. Conditioning

¹²Furthermore, we show in Section 5 below that even among Ivy-Plus students, those with higher test scores and academic ratings are far more likely to obtain jobs at elite or prestigious firms. These correlations further support the view that these firms are viewed as desirable by those who have broad options rather than simply capturing which firms happen to be more popular among Ivy-Plus graduates than flagship public graduates.

on attendance amplifies the differences in characteristics and outcomes between the three groups. The characteristics and outcomes of individuals in the college-specific analysis samples are generally similar to those in the pipeline samples for the same groups of colleges (which include all colleges in the relevant groups), showing that the subset of colleges for which we have internal admissions records are broadly representative of the colleges in their tier.

3 College Attendance by Parental Income: Pipeline Analysis

Why are children from high-income families more likely to attend highly selective private colleges? In this section, we answer this question in three steps. We first characterize how attendance rates vary with parental income for children with similar pre-college qualifications. We then decompose the college attendance pipeline into three parts—applications, admissions, and matriculation—and quantify how much each contributes to income gaps in attendance rates. Finally, after establishing that differences in admissions rates are a key driver of the gaps, we characterize the specific admissions practices that lead to higher admissions rates for children from high-income families. Throughout this section, we use our pipeline analysis sample – students who are on pace to graduate high school in 2011, 2013, or 2015.

3.1 Attendance Rates Conditional on Test Scores

To understand the degree to which colleges can change the socioeconomic diversity of their student bodies, we begin by separating disparities that emerge prior to college application from those that emerge during the college application and admissions process. Following prior work, we use standardized (SAT and ACT) test scores as a proxy for academic credentials at the point of college application.¹³ Test scores differ sharply by parental income, with children from high-income families having much greater chances of scoring at the top of the distribution than those from lower-income families (Appendix Table 3), consistent with substantial socioeconomic disparities in childhood environments and education prior to college application.

Even holding fixed test scores, however, there are still large differences in students' chances of attending Ivy-Plus colleges by parental income. Figure 2a plots Ivy-Plus attendance rates for students scoring at the 99th percentile on standardized tests (an SAT score of exactly 1510 or an ACT score of 34). Among these high-scoring students, more than 30% who come from families in the top 1% (income > \$611k) attend Ivy-Plus colleges. In contrast, just 10% of students scoring at the 99th percentile from families in the “middle class” of the applicant pool (between the 70th to 80th percentile of the national income distribution) attend Ivy-Plus colleges.¹⁴

¹³Although test scores are not perfect measures of either past academic achievement or students' future potential, they provide a consistent baseline measure of pre-college qualifications that is available for nearly all students during the period we study, when standardized tests were required by most selective colleges. The conclusions we draw below do not rely on the assumption that test scores fully capture student potential. We examine how other measures of student credentials vary with parental income in Section 3.3 and then test whether those credentials and test scores predict students' post-college outcomes in Section 6.

¹⁴See Appendix Table 4 for the dollar values corresponding to the quantiles of the parental income distribution plotted in Figure 2a.

One can construct a series analogous to that in Figure 2a for every SAT and ACT score level and every Ivy-Plus college separately. To obtain a single summary measure of how attendance rates vary with parental income controlling for test scores, we take a weighted average of attendance rates by test score in each parental income bin, weighting by the distribution of test scores of students who attend each Ivy-Plus college. We then combine these measures for each of the 12 Ivy-Plus colleges into a single overall mean by taking an enrollment-weighted mean across the 12 colleges and dividing by the overall mean of the resulting series to obtain measures of relative Ivy-Plus attendance rates by parental income controlling for test scores.

The series in green circles in Figure 2b plots the resulting test-score-reweighted average Ivy-Plus college attendance rates using our pipeline analysis sample. Consistent with the pattern in Figure 2a, students from the top 1% are 2.3 times more likely to attend an Ivy-Plus college than students from the middle class (p70-80) with comparable test scores, averaging across test score levels. Students from the bottom 40% of the income distribution have slightly higher Ivy-Plus attendance rates than students from the middle class with the same test scores. The result is a “missing middle” pattern where attendance rates are lowest conditional on SAT/ACT scores for middle-class students.¹⁵ These differences in attendance rates by parental income remained stable between the entering classes of 1998-2018 (Appendix Figure 2a). Note that these differences in attendance rates by parental income do not arise from differences in attendance rates by race and ethnicity: reweighting to hold both the distribution of test scores and race and ethnicity constant across parent income bins yields similar results (Appendix Figure 3a).

For comparison, Figure 2b also plots test-score-controlled attendance rates, constructed using the same reweighting approach, for the 12 other highly selective private colleges in our sample and the nine state flagship colleges (listed in Appendix Table 1). Other highly selective private colleges exhibit a similar pattern to Ivy-Plus colleges, with much higher attendance rates for students from high-income families and the lowest attendance rates for families from the middle class. In contrast, attendance at flagship public colleges is roughly constant up to the 80th percentile conditional on test scores, then rises by a factor of 1.3 from the 80th to 95th percentiles, and is roughly constant thereafter – not exhibiting the very sharp spike at the top observed at private colleges. The gradient in attendance by parental income at public colleges is driven primarily by differences in the attendance rates of out-of-state students, whose attendance rates vary with parental income in a manner that is similar to that at Ivy-Plus colleges (Appendix Figure 5a).

We find qualitatively similar patterns at each of the colleges within our three groups, although the magnitudes of the gradients differ across colleges (Appendix Figure 6). With the exception of MIT – which exhibits relatively constant attendance rates by parental income – attendance rates at every Ivy-Plus college are significantly higher conditional on test scores for students from families in the top 1%. The same is true among highly selective private colleges, with the exception of Cal Tech and Carnegie Mellon (Appendix Figure 6b). Public colleges all exhibit shallower gradients, with the exception of the University of Michigan, Ann Arbor, where attendance rates rise sharply for high-income students (Appendix Figure 6c), primarily

¹⁵The “missing middle” in this context consists of families that earn well above the national median income; for instance, families earning in the 70th to 80th percentile of parents for these cohorts earn between \$90,000 and \$120,000.

driven by out-of-state enrollment (Appendix Figure 6e).

Quantifying the Number of Extra High-Income Students. We quantify the impact of these differences in attendance rates on the socioeconomic composition of the student body by considering a counterfactual scenario in which students from the top 1% attend Ivy-Plus colleges at the same rates as students from the 70th-80th percentiles with the same test scores (see Appendix F for details). For each college c , we define

$$\text{Counterfactual Attendance Rate}_c = \sum_a N_{Top\,1\%,a} \times \text{Attendance Rate}_{P70-80,ac}, \quad (1)$$

where $N_{Top\,1\%,a}$ denotes the number of test takers with a score of a from the top 1% and $\text{Attendance Rate}_{P70-80,ac}$ denotes the fraction of students who attend college c among students with score a from the 70th-80th percentiles. We then scale the resulting counterfactual attendance rate to a class of 1650 students, which is approximately the average number of entering first-year students at Ivy-Plus colleges in Fall 2022.

Under this counterfactual of “income neutral” attendance conditional on SAT/ACT scores, there would be 93 students from the top 1% in the average Ivy-Plus class – 168 fewer than the 261 students from the top 1% we observe in our data for the average Ivy-Plus college in our pipeline analysis sample (Table 2). Put differently, there are 168 “extra” students from the top 1% (10.2% of total enrollment) relative to what one would expect based on their SAT and ACT scores.

Mechanically, 1% of the Ivy-Plus class (16 students) would come from families in the top 1% under an unconditionally income-neutral benchmark with equal representation across all income groups. There are an additional 77 top 1% students in our “income neutral conditional on test scores” benchmark because there are large differences in test scores by parental income, presumably due to differences in childhood environments and schools; for instance, 10.4% of students who score above 1500 on the SAT come from families in the top 1% (Appendix Table 3). Relative to the unconditional income neutral benchmark, there are 245 extra students from the top 1% in the average actual Ivy-Plus class; of these 245, 168 (69%) are accounted for by differences in attendance rates conditional on test scores, while 77 (31%) come from differences in test scores themselves. Hence, much of the over-representation of the top 1% in Ivy-Plus colleges can be reduced by addressing differences that emerge in the college application and admission process rather than pre-college-application factors. The rest of this section seeks to understand what parts of the college application and admissions process account for the additional 168 students from top 1% families conditional on test scores.

3.2 Applications, Admissions, and Matriculation Rates

We now decompose the differences in attendance rates by parental income conditional on test scores into the effects of application, admissions, and matriculation. How much does each of these margins contribute to the additional 168 students from the top 1%?

Applications. We measure application rates in our full pipeline analysis sample using data from testing companies on the colleges to which students sent their test scores. Sending a test score to a college is an informative but noisy proxy for application, as some students may send test scores but not ultimately apply and vice versa. We adjust for the noise in score sending by building a prediction model using the subset

of colleges in our college-specific sample for which we observe true application data from colleges' internal records (see Appendix C).

Figure 3a shows predicted application rates by parental income group (normalized by the overall mean), controlling for test scores by reweighting students in each income bin to match the distribution of attendees' test scores as above. Children from high-income families apply at 37% higher rates to Ivy-Plus colleges than those from middle-class families with comparable test scores, while those at the lowest income levels apply to Ivy-Plus colleges at 19% higher rates than those in the middle class. While there are some colleges where application rates vary more sharply with parent income – such as Dartmouth and Duke – at every Ivy-Plus college, the difference in application rates by parental income is considerably smaller than the difference in attendance rates (Appendix Figure 7a).

Application rates vary more sharply with parental income at other colleges. Selective private colleges receive 85% more applications from students in the top 1% than students with comparable test scores in the middle class, with even larger differences at certain colleges such as Georgetown and Vanderbilt (Appendix Figure 7b). Flagship public colleges receive 62% more applications from students in the top 1% than students in the middle class, a gradient that is again driven primarily by out-of-state applicants (Appendix Figure 5b).

Since Ivy-Plus application rates do not exhibit the same spike at the top of the income distribution that attendance rates do, the gradient in attendance rates must be driven by differences in either admission or matriculation rates. Unfortunately, we cannot distinguish between admissions and matriculation in our full sample of colleges. We therefore turn to our college-specific subsample, where we have data from admissions offices on admissions and matriculation decisions, for the rest of our pipeline analysis.¹⁶

Admissions. Figure 3b plots admissions rates by parental income for applicants to the Ivy-Plus and flagship public colleges in our college-specific sample. As above, we reweight within each income bin to match the test score distribution at each college and divide the resulting rates by each college's overall mean admission rate.

Admissions rates are substantially higher for applicants from the highest-income families at Ivy-Plus colleges. Students with parental incomes in the top 0.1% are 2.5 times more likely to be admitted than students from the middle class (p70-80) with comparable test scores. Students with parental incomes in the 99-99.9 percentile are 44% more likely to be admitted than students from the middle class. In contrast, admission rates at the five flagship public universities in our college-specific sample are essentially constant across the income distribution. The differences in admissions rates by parental income again persist after controlling for differences across racial and ethnic groups (Appendix Figure 3b).

Matriculation. Figure 3c plots matriculation rates of admitted students at selected Ivy-Plus colleges and

¹⁶These colleges are representative of Ivy-Plus colleges in their attendance patterns: the (equal-weighted) average attendance rate conditional on application is 1.7 times higher for students from the top 1% than for those from the middle class (Appendix Figure 4); the average of the ratio for the colleges we have internal data from is approximately 1.8. Furthermore, each of the colleges from which we have data exhibits similar patterns individually to the average results we report below, further supporting the view that the findings from this sample apply across Ivy-Plus colleges.

flagship publics by parental income, again controlling for test scores by reweighting as above. Applicants with parental incomes in the top 1% are 1.15 times more likely to attend Ivy-Plus colleges once admitted than those from the middle class. Most of this gradient arises from differences between the matriculation rates of those students admitted in early vs. regular admissions rounds; high-income students are more likely to apply and be admitted in the early admissions round, where matriculation rates are higher (Appendix Figure 9). Flagship publics display a similarly flat pattern across the income distribution, with high-income students exhibiting slightly lower matriculation rates.

Quantification. The preceding analysis suggests that differences in admissions—rather than application or matriculation—drive most of the gap in attendance rates by parental income at Ivy-Plus institutions. We now quantify the relative importance of these sources in explaining the “extra” 168 students from the top 1% who attend Ivy-Plus colleges (see Appendix F for details on our methods).

We begin by focusing on students who are not recruited athletes. Athletes are typically invited to apply to Ivy-Plus colleges only if they are likely to be accepted and have committed to attend, making it difficult to quantify the relative importance of application, admissions, and matriculation for them. We first calculate how many non-athletic applicants from the top 1% would attend Ivy-Plus colleges if their admissions rates conditional on test scores were the same as those for middle-class students:

$$\text{Equal Admit CF}_c = \sum_a N_{Top\ 1\%,a} \times \text{Application Rate}_{Top\ 1\%,ac} \times \text{Admission Rate}_{P70-80,ac} \times \text{Matriculation Rate}_{Top\ 1\%,ac} \quad (2)$$

We then further equalize matriculation rates, and then application rates, at which point (mechanically) the attendance rate for top 1% students is equal to that of middle-class students with the same test scores. Using this approach, 87 out of the 168 extra top 1% students can be accounted for by the higher admissions rates of non-recruited-athletes from the top 1% (Table 2).¹⁷ If colleges were to then further eliminate the differences in matriculation rates by income – e.g., by addressing differences that arise between early and regular application rounds – the number of students from the top 1% would fall by a further 20 students. Finally, equating application rates across the income distribution would reduce the number of students from the top 1% by an additional 34 students. Together, the three components of the pipeline for non-athletes account for 141 of the 168 extra students from the top 1%.

The rest of the extra students from the top 1% come via athletic recruitment. Figure 4a illustrates this by plotting the fraction of students admitted to Ivy-Plus colleges who are recruited athletes by parent income level. The share of recruited athletes rises from 5% for students from the bottom 60% of the parental income distribution to 13% for students from the top 1%, with a slightly steeper income gradient for females relatives to males (Appendix Figure 10). The disproportionate share of athletes from high-income families

¹⁷The same counterfactual of equal admissions rates conditional on SAT scores would also reduce the number of students from low-income families (Appendix Table 5) because they too are admitted at higher rates relative to the middle class conditional on test scores. However, the magnitude of this effect is much smaller than on the top 1%: for instance, the number of students from families in the bottom 20% would fall by 16. This is because there are many fewer students with sufficiently high test scores to be admitted to Ivy-Plus colleges at lower parental income levels, as shown in Appendix Table 3.

among admitted students contributes another 27 extra top 1% students.¹⁸ In contrast, at flagship public colleges, the share of recruited athletes is constant across the income distribution (Figure 4b).

Our calculations imply that two-thirds of the extra top 1% students (114 out of 168) can be accounted for by Ivy-Plus colleges' admissions practices, with 87 coming from higher admissions rates for non-athletes and 27 coming from athletic recruitment.¹⁹

3.3 Determinants of Admissions Rates at Ivy-Plus Colleges

Why are applicants from high-income families admitted to highly selective private colleges at higher rates, conditional on having the same test scores? In this section, we identify the mechanisms underlying this admissions advantage. We exclude recruited athletes throughout this subsection, since their path to admission is distinct from other students as discussed above.

Legacy Preferences. It is well established that legacy students – students whose parent(s) attended the college to which they apply – receive special consideration in college admissions (Espenshade, Chung, and Walling 2004; Bowen, Kurzweil, and Tobin 2006; Hurwitz 2011; Arcidiacono, Kinsler, and Ransom 2022). However, prior studies have not measured the extent to which legacy preferences contribute to higher admissions (and ultimately attendance) rates for students from high-income families.

The effect of legacy preferences on differences in admissions rates by parent income depends on two factors: (1) the extent to which these students come from high-income families and (2) the extent of the admissions preference for legacy students. Figure 5a characterizes the first factor, plotting the share of legacy applicants by parental income group, reweighted on test scores across parental income groups as above.²⁰ Overall, legacy applicants constitute 2.5% of the applicant pool. This fraction rises monotonically with parental income, rising to more than 9% for applicants from the top 1%.

The series in green dots in Figure 5b characterizes the second factor by plotting admission rates for legacy students, reweighted to match the test score distribution of Ivy-Plus attendees as above, divided by the mean (test-score-reweighted) admission rate for all applicants. Admissions rates are considerably higher for legacy applicants relative to an average applicant with comparable test scores, especially at higher income levels: legacy applicants from the top 1% have more than a 5-fold advantage in admissions, as compared with a 3-fold advantage at lower income levels.²¹

¹⁸To calculate the number of extra top 1% students due to athlete recruitment, we adjust the fraction of athletes from the top 1% to match that from the middle class in the new counterfactual student body, after equating application, admissions, and matriculation rates across parental income groups for non-athletes conditional on test scores as above. This effectively assumes that colleges recruit athletes across the income distribution in proportion to the number of students with test scores comparable to those currently enrolled at Ivy-Plus colleges.

¹⁹Because (2) is multiplicative, the results of this decomposition analysis depend upon the order in which one changes each of the three margins. In Appendix Table 6 we present a decomposition that averages across the different orders in which the three margins (admissions, application, matriculation) could be changed. Averaging across orderings, admissions still account for 58% (96 students) of the overall gap in attendance. Our counterfactuals also consider changes in policies at a single Ivy-Plus college. In practice, the admissions advantages enjoyed by top 1% students may increase their admissions rates at many different colleges Ivy-Plus; modeling how such interactions across colleges would affect the counterfactuals considered here is an interesting direction for future work.

²⁰See Appendix Figure 11b for statistics on legacy shares and admissions rates by parent income that do not control for test scores.

²¹The legacy advantage is larger for applicants from high-income families partly because there is a complementarity between the legacy boost and other factors that correlate with parental income, such as non-academic ratings, attending a private high

The higher admissions rates for legacy students in Figure 5b could be driven by a preference for children of alumni themselves or because legacy students have stronger academic or non-academic credentials relative to other students with the same test scores. We use two approaches to distinguish between these hypotheses and isolate the effect of legacy status itself.

We first use the rich set of variables in our admissions data to account for observable differences in legacy applicants' credentials. To do so, we first predict admissions separately for non-legacy and legacy students using OLS regressions of an indicator for admission on fixed effects for all combinations of students' academic and non-academic ratings, application round, entering class, race, first-generation status, gender, parental income group, high school GPA (where available), and high school fixed effects, reweighting observations so that the distribution of test scores in each parental income bin matches that distribution for students attending the relevant college. We then assign legacy applicants a counterfactual non-legacy admissions rate by predicting their admissions rates using admissions model coefficients estimated on the non-legacy sample (retaining the high school fixed effects for the high schools legacies actually attended because the set of high schools does not overlap perfectly across legacy and non-legacy applicants). The dashed line in Figure 5b plots the resulting counterfactual admissions rates for legacy students, taking into account their different credentials but ignoring their legacy status. Even absent legacy preferences, children of alumni would be admitted at slightly higher rates than non-legacy students because of their favorable observable characteristics (stronger academic credentials, etc.). However, these counterfactual admissions rates are only slightly higher than for non-legacy students, implying that most of the roughly 4-fold difference on average between the observed admissions rates of legacy and non-legacy students is due to the effect of legacy preferences themselves. This 4-fold legacy admissions advantage is comparable to the implied admissions advantage given to recruited athletes based on the same admissions model.

The preceding approach relies on a “selection on observables” assumption – namely that controlling for the factors observed in application files is adequate to account for the different attributes of legacy applicants. There may be other characteristics of legacy applicants not recorded in the data (such as the nature of their recommendation letters or activities) that explain their higher admissions rate. To address such unobservables, we turn to a second approach: comparing admissions rates for legacy applicants at the college their parents attended to their admissions rates at *other* Ivy-Plus colleges. If legacy applicants have stronger unobserved credentials, they should have higher admissions rates at all Ivy-Plus colleges, not just the particular college their parents attended.

To implement this test, we focus on individuals who applied to at least two Ivy-Plus colleges in our college-specific sample and compare admissions rates controlling for the same vector of variables as those used in the admissions model described above.²² Figure 5c shows that legacy students are accepted at four

school, etc. The differences in admissions rates lead to similar differences in attendance rates between legacy and non-legacy students, taking differences in matriculation rates across students into account (Appendix Figure 12).

²²We exclude students who applied early to one of the colleges from this analysis since the decision to apply to another college is endogenous to the admissions decision at the college to which the student applied early. We control parametrically for test scores instead of reweighting here to maximize precision in the smaller sample of legacy applicants who apply to two or more colleges; using parametric controls instead of non-parametric reweighting yields very similar estimates in the full sample

times the rate of non-legacy applicants with comparable test scores at the college their parents attended, but are only slightly more likely to be admitted than other applicants at other colleges.²³ Furthermore, the predicted counterfactual admissions rate for legacy students at other colleges is very similar to the actual admissions rate for those students, providing an out-of-sample validation of our predictions based on observable characteristics.

Using our counterfactual predictions, we estimate that legacy preferences (holding fixed all other credentials) lead to 52 additional students from the top 1% (Table 2).²⁴

Application Credentials. To understand the source of the remaining 35 extra students from the top 1%, we examine how application credentials, measured along both academic and non-academic dimensions by numerical ratings assigned by admissions officers, vary with parental income. We focus on one of the Ivy-Plus colleges for which we have the most granular data.²⁵ To separate the effects of other factors from legacy preferences, we exclude legacy applicants (and children of faculty) from this analysis.

We begin by analyzing how academic credentials vary with parental income. One potential explanation for the higher admissions rates of high-income students is that they have stronger overall academic credentials conditional on their SAT/ACT scores. For example, students from high-income families may have higher grade point averages, taken a more difficult curriculum in high school, or achieved other academic distinctions (e.g., in science fairs or math competitions). To test this hypothesis, Figure 6a plots the fraction of students who obtain a high academic rating – defined as having ratings in the top 40% of the applicant pool – by parental income, again reweighting observations so that the distribution of test scores in each parental income bin matches that distribution for attending students. The share of applicants who obtain high academic ratings is essentially constant across the parental income distribution, and is in fact slightly *lower* for students from the top 1% of the income distribution than for those from the upper-middle class.

In contrast, Figure 6b shows that students from the top 1% of the income distribution are significantly more likely to have strong *non-academic* ratings (for participation in extracurricular activities or leadership traits) as compared with students from the bottom 99 percent. The gap in non-academic ratings by parental income grows with students' test scores; that is, students with the strongest non-academic credentials tend to be those who have strong academic records *and* come from high-income families (Appendix Figure 15).

Students from the top 1% are also more likely to obtain higher ratings on the strength of their teacher analyses above.

²³One concern with this test is that colleges may choose not to admit applicants whose parents attended higher-ranked colleges, since such a student is less likely to attend if admitted. Appendix Figure 13a addresses this concern by testing whether legacies at a lower-ranked reference school (based on a revealed-preference ranking) are admitted to higher-ranked colleges. The average admissions rates are lower in this more selective sample of other colleges, but the gap between admissions rates for students who are legacies and non-legacies at the lower-ranked reference school remains similar.

²⁴We also replicate the preceding analysis on students who are children of faculty at the institution to which they apply. Although these students are admitted at even higher rates than legacy students with comparable credentials (as shown by Arcidiacono, Kinsler, and Ransom 2022), the admissions advantage for faculty children results in less than half an extra student on average from the top 1% because children of faculty account for only 0.1% of applicants Ivy-Plus colleges (Table 1).

²⁵Numerical ratings are commonly used at Ivy-Plus colleges, but the granularity of the ratings varies. Some colleges use a 3 point scale (high, medium, low) while others use finer gradations and the specific categories vary as well. Although our primary analysis focuses on only one college for which we observe the most detailed, granular ratings, we show in Appendix Figure 14 that when we coarsen the ratings at this college to match the data available elsewhere, we obtain similar qualitative results across the colleges in our college-specific sample.

recommendation and guidance counselor letters (Appendix Figure 16) – two factors that contribute to non-academic ratings – suggesting that high schools may play a key role in explaining why students from high-income families have higher non-academic ratings. Indeed, we show in Appendix G that students who attend non-religious private high schools receive higher non-academic ratings and are admitted to Ivy-Plus colleges at twice the rate of students with comparable test scores and demographics who attend well-resourced public high schools (Appendix Figure 17c). Because children from high-income families are more likely to attend private high schools, these differences across high schools contribute to differences in outcomes by parental income (Appendix Figure 18a).

We quantify the contribution of non-academic ratings to the admissions advantage for top 1% students by returning to the parametric admissions model used to quantify the legacy effect above. We use the model estimated on non-legacy students to calculate how the number of admitted students from the top 1% would change if they received the same distribution of ratings as students from the middle class (p70-80) with the same standardized test scores. This further lowers the number of admitted students from the top 1% of the income distribution by 35 (Table 2), accounting for the remaining “extra” top 1% non-athlete students due to admissions.²⁶

In summary, two-thirds of the higher Ivy-Plus attendance rates of students from the top 1% relative to the middle class is explained by an admissions advantage that arises from three factors: athletic recruitment, legacy preferences, and higher non-academic ratings. These findings also explain why public colleges exhibit much smaller differences in admissions rates by parental income. Public colleges typically do not consider legacy status, have a much smaller share of recruited athletes in their student bodies, and use more standardized processes to evaluate applications (or holistic reviews processes with less emphasis on non-academic factors). The holistic admissions processes used by private colleges create incentives and scope for students from high-income families to further differentiate themselves from others (e.g., by enrolling at private high schools that provide non-academic credentialing).

4 Causal Effects on Post-College Outcomes

How would admitting more low- and middle-income students to Ivy-Plus colleges change their post-college outcomes? In this section, we estimate the causal effect of attending an Ivy-Plus college instead of an average flagship public college on students’ post-college outcomes using two research designs. The first isolates idiosyncratic variation in admissions decisions, while the second exploits variation in where students choose to matriculate conditional on their admissions portfolios. Throughout this section, we focus on the subset of Ivy-Plus colleges for which we have internal admissions records, which are representative of Ivy-Plus

²⁶The fact that legacy preferences and higher non-academic ratings fully account for the non-athlete high-income admissions advantage is not the result of a mechanical decomposition; it just turns out empirically that these two factors fully explain the observed difference in admissions rates. Consistent with these conclusions, reweighting on non-academic ratings or controlling for high school fixed effects nearly eliminates the top 1% admissions advantage conditional on test scores (Appendix Figure 19b).

colleges more broadly in terms of their causal effects.²⁷

We begin by presenting a statistical model to specify the two research designs and their identification assumptions. We then establish a set of surrogate outcomes that serve as proxies for the leadership outcomes of ultimate interest. Finally, we report treatment effects on the surrogate outcomes from the two research designs.

4.1 Statistical Model

4.1.1 Setup

As discussed above, enrolling in a selective college in the U.S. involves three steps: application, admission, and matriculation. Because our research designs start by conditioning on the set of colleges to which students apply, we take the application set as exogenous and begin by modeling colleges' admissions decisions.

College Admissions. Each college j assigns applicant i a rating

$$Z_{ij} = \gamma_{1j} X_{1i} + \gamma_{2j} X_{2i} + \eta_i + \epsilon_{ij}, \quad (3)$$

where X_{1i} denotes a characteristic of student i that we observe in our data (e.g., her SAT/ACT test score) and X_{2i} denotes an unobservable characteristic (e.g., an admissions committee's assessment of a student's motivation) that may be correlated with the student's post-college outcome Y_i (e.g., earnings or one of the leadership outcomes in Figure 1). The relative weights placed on these components, controlled by γ_{1j} and γ_{2j} , may vary across colleges. Students' ratings also depend upon two other unobserved components that are uncorrelated with potential outcomes: a component η_i that is common across colleges (e.g., having a guidance counselor who writes an especially strong letter of support for a student with given characteristics) and a component ϵ_{ij} that is uncorrelated across colleges (e.g., idiosyncratic noise in different reviewers' assessments of the same letters, or whether the student happens to play a musical instrument needed in college j 's orchestra in the year they apply). Assume that ϵ_{ij} has infinite support, a regularity condition that ensures that any candidate has some non-zero probability of admission to a college j .

Colleges admit student i if $Z_{ij} > C_j$, where C_j denotes a college-specific cutoff for admissions. Note that this structure assumes that colleges do not condition their admissions decisions for student i on his or her admissions outcomes at other schools. Let P_{ij} denote an indicator for whether student i is admitted to college j . Let J_i denote the set of colleges to which student i is admitted and D_{ij} denote an indicator for whether student i chooses to enroll in college j , so that $D_{ij} = 1$ for one college $j \in J_i$ and $D_{ij} = 0$ for all others.

Post-College Outcomes. Students' post-college outcomes Y_i are a function of the same characteristics that enter colleges' ratings (X_1 and X_2), idiosyncratic noise ϵ_i^Y , and college-specific value-added:

²⁷Observational value-added models estimated (as described below) in the pipeline analysis sample imply a 6.3 pp increase in the predicted probability of reaching the top 1% from attending an Ivy-Plus college instead of the average flagship public, averaging across all 12 Ivy-Plus colleges; the corresponding difference in value-added for the subset of Ivy-Plus colleges we study below relative to state flagships is approximately 6.6 pp.

$$Y_i = \sum_{j \in J_i} D_{ij} \phi_j + \beta_1 X_{1i} + \beta_2 X_{2i} + \epsilon_i^Y, \quad (4)$$

where ϕ_j denotes college j 's causal effect (value-added) on Y_i . This model assumes that college value-added ϕ_j is homogeneous across students; we present evidence that this is a good approximation for the set of Ivy-Plus colleges and applicants we study by showing that colleges' causal effects are similar across subgroups and different margins of admission or enrollment.

We normalize the value-added of the outside option (denoted by college O) to $\phi_O = 0$ and assume for simplicity that everyone in the sample applies and is admitted to the outside option college ($P_{iO} = 1$ for all i). Note that by definition, the error terms η_i and ϵ_{ij} in admissions ratings are orthogonal to the error term in students' long-term outcomes ϵ_i^Y ($Cov(\epsilon_{ij}, \epsilon_i^Y) = Cov(\eta_i, \epsilon_i^Y) = 0$), since unobservable factors that affect both admissions and long-term outcomes are captured in X_{2i} .

Our goal is to estimate ϕ_A , the treatment effect of attending an Ivy-Plus college (denoted by college A) instead of the outside option (college O), which we define as the average flagship public college in our college-specific sample (i.e., the 9 colleges listed in Appendix Table 1).

As discussed in Dale and Krueger (2002), simply comparing the outcomes of students who attend college A vs. O conditional on observable characteristics X_{1i} , $E[Y_i|D_{iA} = 1, X_{1i}] - E[Y_i|D_{iO} = 1, X_{1i}]$, yields a biased estimate of ϕ_A because the omitted variable X_{2i} affects both the probability of admission to college A and the outcome Y_i . We now discuss two research designs that yield unbiased estimates of ϕ_A by making use of additional data under different identification assumptions.

4.1.2 Research Design 1: Isolating Idiosyncratic Variation in Admissions

Our first research design makes use of additional information \tilde{X}_{2i} from college A 's admissions files – in particular, whether the admissions committee places the candidate on the waitlist for admission – to isolate idiosyncratic variation in admissions decisions that is plausibly orthogonal to students' long-term potential outcomes. We view \tilde{X}_{2i} as a potentially imperfect proxy for the (unobserved) student characteristic X_{2i} :

$$X_{2i} = \tilde{X}_{2i} + \epsilon_{2i}^X$$

Consider the difference in expected earnings between students admitted vs. rejected by college A , controlling now for both X_{1i} and \tilde{X}_{2i} , divided by the probability of matriculating conditional on admission:

$$r_A = (E[Y_i|P_{iA} = 1, X_{1i}, \tilde{X}_{2i}] - E[Y_i|P_{iA} = 0, X_{1i}, \tilde{X}_{2i}])/E[D_{iA}|P_{iA} = 1, X_{1i}, \tilde{X}_{2i}] \quad (5)$$

If $Var(\epsilon_{2i}^X) = 0$, then $X_{2i} = \tilde{X}_{2i}$, and it follows that this rescaled difference in conditional means is an unbiased estimate of the causal effect of attending college A instead of O (i.e., $r_A = \phi_A$) for a student who applies only to colleges O and A .²⁸

²⁸When students apply to multiple colleges, the reduced-form comparison between applicants admitted vs. rejected from college A will capture the difference between ϕ_A and the average value-added of the college that students attend when rejected

Equation (5) can be interpreted as a conventional instrumental variables (IV) estimator, where the endogenous variable is enrolling at college A (instead of O) and the instrument in being admitted to college A , among the sample of applicants waitlisted for admission at college A . As in standard IV estimators, identification requires a relevance condition (that the denominator of (5) is not zero) and an exclusion restriction (that admission has no direct relationship with outcomes, independent of its effect on attendance). Relevance holds mechanically: since students who are not admitted cannot attend, and the vast majority of candidates admitted to Ivy-Plus colleges choose to attend, the first stage F statistic in (5) exceeds 1,000. Exclusion in this context can be broken into two requirements. The first is that admission to an Ivy-Plus college itself has no impact on a student's outcomes if the student does not attend; we view this as a reasonable assumption since other channels unrelated to college attendance (e.g., impacts on confidence or signaling likely have modest impacts relative to the direct treatment effect of attendance identified below. The second, which is the key identification assumption, is that the unobservable determinants of long-term outcomes are balanced for students admitted vs. rejected from the waitlist, i.e., that the proxy \tilde{X}_{2i} fully captures the variance in X_{2i} .

We test whether this key identification assumption holds using a new “multiple-rater” test, exploiting the fact that we observe independent admissions decisions at other colleges. Our approach relies on the following assumption about college's admissions decisions.

Assumption 1 (Correlated Admissions Criteria). *Any unobserved component of students' applications associated with long-term outcomes that affects admissions at college A affects admissions at another college B with the same sign: $\gamma_{2A} > 0 \Rightarrow \gamma_{2B} > 0$*

Let the difference in an applicant's probability of admission to college B conditional on being accepted vs. rejected at college A be given by:

$$T_{B|A} = E[P_{iB} = 1 | P_{iA} = 1, X_{1i}, \tilde{X}_{2i}] - E[P_{iB} = 1 | P_{iA} = 0, X_{1i}, \tilde{X}_{2i}].$$

Under Assumption 1, if admissions decisions at college B are orthogonal to those at college A conditional on X_{1i} and \tilde{X}_{2i} , then (5) yields unbiased estimates of the causal effect of admission to A :

$$T_{B|A} = 0 \implies r_A = \phi_A.$$

The intuition underlying this result (which we prove formally in Appendix H) is straightforward: if colleges' decisions are based on the same latent factors that predict long-term outcomes, any residual variation in such latent factors (conditional on the controls \tilde{X}_i) will lead to correlations in admissions decisions. If no such correlation exists, the variation in admissions decisions A in the marginal pool (i.e., conditional on the controls) must be due to idiosyncratic factors unrelated to long-term outcomes.

It is instructive to consider two cases where the key correlated admissions assumption fails. First, suppose that college B follows a rule-based admissions procedure that considers only the observable factor X_1 (e.g.,

from A . We address this complication by estimating reduced-form treatment effects heterogeneously by students' outside options in order to estimate ϕ_A , the effect of attending A relative to the average flagship public college (see Section 4.3).

standardized test scores), whereas college A also considers unobservable factors X_2 that may be correlated with long-term outcomes. In this case, $\gamma_{2B} = 0$, and our test fails: even though admissions decisions at B may be uncorrelated with those at A , those who are admitted at A may have different potential outcomes from those who are rejected. To address this issue, we focus on admissions decisions at other Ivy-Plus colleges with similar admissions procedures to estimate the test statistic $T_{B|A}$.

Second, consider two colleges that both consider unobservable criteria but put weight on different factors. For example, suppose that college A puts weight on unobserved measures of mathematical ability, while college B puts weight on unobserved measures of artistic ability, and assume those two factors are uncorrelated with each other but are both correlated with long-term outcomes. In this case, our test would fail once again. While we cannot directly measure all the latent factors that colleges may consider, to rule out such a scenario, we find that for the subset of variables \tilde{X}_{2i} that we do observe, attributes that are correlated with long-term outcomes and are positively associated with admissions and ratings at one Ivy-Plus college are also associated with admissions at other Ivy-Plus colleges controlling for test scores and parental income (Appendix Figure 20), supporting the validity of our assumption.

4.1.3 Research Design 2: Isolating Idiosyncratic Variation in Matriculation

Our second research design isolates variation in matriculation decisions that may be orthogonal to students' potential outcomes by controlling for the set of colleges to which students are admitted, as in Dale and Krueger (2002) and Mountjoy and Hickman (2021). Consider the difference in expected outcomes (controlling for X_{1i}) between students admitted to the same set of colleges, but who choose to attend different colleges:

$$r_M = E[Y_i|D_{iA} = 1, X_{1i}, J_i = \{A, O\}] - E[Y_i|D_{iO} = 1, X_{1i}, J_i = \{A, O\}] \quad (6)$$

Assume that controlling for the set of colleges to which a student is admitted eliminates any correlation between a student's potential outcomes and her choice of which college to attend.

Assumption 2 (Admissions Portfolios Capture Selection). *Conditional on the set of colleges to which a student is admitted and her observable characteristics X_{1i} , unobserved determinants of student i 's long-term potential outcomes are orthogonal to which college she chooses to attend: $E[X_{2i}|D_{ij}, J_i, X_{1i}] = E[X_{2i}|J_i, X_{1i}]$.*

Assumption 2 (which is equivalent to Assumption 1 in Mountjoy and Hickman, 2021) immediately implies that $r_M = \phi_A$ (recalling that ϕ_O has been normalized to 0), since $E[X_{2i}|D_{iA} = 1, X_{1i}, J_i] = E[X_{2i}|D_{iO} = 1, X_{1i}, J_i]$. Under this assumption, two students i and i' who are both admitted to colleges A and O but choose to attend different colleges have comparable potential outcomes, and thus the difference in their expected outcomes reveals the relative value-added of college A .

If students select colleges in a manner correlated with their potential outcomes – e.g., if students who expect to have better long-term outcomes forego paying the potentially higher cost of attending the more selective college A even after being admitted – then Assumption 2 would fail and (6) would yield biased estimates of ϕ_A . While we cannot directly test Assumption 2, we present evidence supporting its validity

by showing that other observables \tilde{X}_{2i} are balanced across students who choose different colleges within a given application set, as in Mountjoy and Hickman (2021).

Both of our research designs use data on admissions decisions at multiple colleges to address selection bias, leveraging the fact that admissions officials observe the factors that are unobservable to the econometrician. The idiosyncratic admissions design uses other admissions decisions to test for selection and isolate idiosyncratic variation in admissions, while the matriculation design uses other admissions decisions to control for selection and isolate idiosyncratic variation in matriculation decisions. When both Assumption 1 and Assumption 2 hold, the two designs yield the same estimates in the simple model above where the returns to college attendance are not heterogeneous (i.e., ϕ_A is constant across students). However, when returns are heterogeneous across students, the two designs identify different local average treatment effects. Our first design identifies the treatment effect of attending college A for students who are narrowly admitted to vs. rejected at college A because of idiosyncratic variation in college A 's assessment of their applications. The second design identifies the return to attending college A instead of O for the subset of students admitted to both colleges who choose to make different choices because of idiosyncratic variation in their preferences over colleges. Because our pipeline analysis above shows that the admissions margin is most central in driving the under-representation of lower-income students at Ivy-Plus colleges, we focus on estimates from the idiosyncratic admissions research design and use the matriculation design to reconcile our findings with prior results.

4.2 Early Career Predictors of Leadership Outcomes

We would ideally implement the research designs described above using the leadership outcomes in Figure 1 as the outcomes Y_i . Unfortunately, we only observe outcomes up to age 33 for students in our college-specific analysis sample, and very few individuals rise to such leadership positions by that age. To address this censoring problem, we use three early-career measures of success as proxies for the primary outcomes of interest: reaching the top 1% of the income distribution at age 33, attending an elite graduate school, and working at a prestigious firm (as defined in Section 2.3).

Prior work has shown that incomes at age 33 are highly predictive of incomes at later ages, and are as predictive of total lifetime income as income measured at any other age (Haider and Solon 2006). This evidence supports the use of income at age 33 as a proxy for achieving a high level of lifetime income.

To evaluate the predictive content of our proxies for other non-monetary leadership outcomes, we use external data sources on the backgrounds of leaders, such as biographical information on public leaders and Nobel laureates (see Appendix I for details). Let Y_i^P denote a primary leadership outcome of interest (e.g., becoming a CEO) and Y_i^S denote an early-career outcome (e.g., working at a prestigious firm). We estimate how the odds of achieving the primary leadership outcome change as a result of achieving the early-career outcome among students who attended Ivy-Plus or flagship public colleges:

$$R_{PS} = \frac{P(Y_i^P | Y_i^S = 1, \text{Ivy-Plus or Flagship Public})}{P(Y_i^P | Y_i^S = 0, \text{Ivy-Plus or Flagship Public})}$$

We report this odds ratio for various leadership outcomes and the early-career proxies for which data are available in Table 3.²⁹ We begin with the financial leadership outcomes considered in the first section of Figure 1. Using information from BoardEx, we estimate that 43.5% of Fortune 500 CEOs who attended Ivy-Plus or flagship public also attended an elite graduate program. This statistic implies that the odds of becoming a Fortune 500 CEO increase by a factor of $R_{PS} = 5.8$ if one attends an elite graduate program. Similarly, individuals who attend elite graduate schools are 9 times more likely to become corporate board or committee members.

To evaluate our proxies for success in the arts and sciences, we focus on Nobel laureates, for whom biographical information about college and graduate student attendance is publicly available from the Nobel website and other sources. Ivy-Plus and flagship public students who attend elite graduate programs are 61 times more likely to become Nobel laureates than those who did not. Finally, we examine leadership in public service, focusing on Supreme Court justices and Treasury secretaries, for whom data on education and early career employment are typically publicly available. Attending an elite graduate school increases the odds of becoming a Supreme court justice by a factor 100 and a Treasury secretary by a factor of 10; working at a prestigious firm doubles the odds of becoming a Supreme court justice and increases the odds of becoming a Treasury secretary by a factor of 4.

Together, these results suggest that the early-career measures of success we are able to observe in our sample are strong predictors of achieving leadership outcomes across a variety of domains. In the rest of this section, we focus on estimating treatment effects on our measures of early career success. We then return to our original goal of predicting the effects of changes in admissions policies on leadership outcomes in Section 6 under assumptions that allow us to translate treatment effects on these short-term proxies to impacts on later outcomes.

4.3 Estimates Based on Idiosyncratic Variation in Admissions

Isolating Idiosyncratic Variation. We identify the treatment effect of attending an Ivy-Plus college for students who would be affected by marginal changes in admissions policies by focusing on applicants placed on admissions waitlists. On average, the Ivy-Plus colleges in our college-specific analysis sample place 10.4% of the applicant pool on the waitlist; of the waitlisted students, 3.3% are ultimately admitted. Admissions decisions from the waitlist are typically made on the basis of differences between expected and actual yield within specific categories where colleges may seek balance, such as by gender, region, in a specific activity such as the orchestra or a sports team, etc. (Clinedinst 2019).

The logic of focusing on waitlisted applicants is similar to that underlying a regression discontinuity (RD) design: waitlisted students are close to the margin of admission and may have similar potential outcomes (i.e., comparable X_{2i}), potentially permitting identification of causal effects of admission by comparing the outcomes of those who are admitted with those who are not. However, since waitlisted applicants are

²⁹Unfortunately, data on early-career incomes are not publicly available and we therefore cannot examine the association between income at age 33 and leadership outcomes.

not admitted randomly (and there is no rank ordering that can be used to implement an RD), there is no guarantee that those who are admitted from the waitlist have the same distribution of unobservables correlated with outcomes X_{2i} as those who are not.

We therefore begin by evaluating whether the variation in admissions decisions among those on the waitlist is driven by idiosyncratic factors ϵ_{ij} that do not affect outcomes or systematic factors X_{2i} that do using the multiple-rater admissions test developed above. Formally, we treat an indicator for being placed on the waitlist as an observable control \tilde{X}_{2i} and test whether the residual variation in admissions conditional on being on the waitlist at a given Ivy-Plus college A is correlated with admissions outcomes at other Ivy-Plus colleges B .

A practical complication in implementing this test is that some colleges make strategic decisions to admit students from their waitlists to manage yield. In particular, a student on the waitlist at a lower-ranked college A may not get in if she was admitted to a higher-ranked college B purely as a result of the admissions decision at college B . This violates the assumption embedded in (3) that colleges make admissions independently and can lead to $T_{B|A} < 0$ even though admission from the waitlist at any given college is orthogonal to potential outcomes. To address this issue, we implement our test using other Ivy-Plus colleges B that are ranked lower (based on revealed preference) by most students relative to the college A whose waitlist decisions we are seeking to evaluate.³⁰

The first column of Figure 7 plots the probability of admission to a given Ivy-Plus college vs. an applicant's admission status at another (lower-ranked) Ivy-Plus college. Individuals who are regular admits at one college have a 50% chance of being admitted at another Ivy-Plus college, while those who are rejected have less than 10% chance of being admitted at another Ivy-Plus college, supporting the correlated admissions criteria assumption underlying our test. Waitlisted candidates' chances of admission to other colleges fall between these two extremes. Among waitlisted candidates, the probability of admission to other colleges does not vary with the admissions outcome: that is, we do not reject the hypothesis that $T_{B|A} = 0$ among waitlisted students.³¹

In the second column of Figure 7, we probe the robustness of this conclusion by controlling for a set of additional observables: a quintic in test scores, parental income indicators (13 dummies corresponding to the

³⁰We identify college rankings based on students' preferred choices when admitted to multiple colleges in our own sample, which accords with the revealed-preference rankings of colleges constructed by Avery et al. (2013). When implementing the test using all colleges rather than just lower-ranked ones, we find, as expected, that the probability of admission to the other college is lower for students who are admitted off the waitlist in the reference college (Appendix Figure 21b). The causal effect estimates we report below using the full sample remain very similar when limiting to the subsample of colleges that pass at least one multiple-rater test with another college (Appendix Figure 22). Furthermore, note that if students admitted from the waitlist at college A are less likely to be admitted to college B than those rejected from the waitlist at college A because they have lower levels of X_{2i} (rather than because of a negative correlation between ϵ_{iA} and ϵ_{iB}), our estimator would underestimate the causal effect of admission to college A .

³¹At the upper bound of the 95% confidence interval, our estimates imply that students admitted from the waitlist are at most 2% more likely to be admitted to other colleges. To gauge the potential bias that could arise from a 2% higher admission rate at other colleges among the admitted pool, note that admitted students at other colleges (among all waitlisted or accepted applicants at those colleges) have a 2 pp higher predicted probability of reaching the top 1%. This 2 pp estimate is an upper bound on the degree to which potential outcomes differ between accepted and rejected applicants on average insofar as the causal effects of admission to any Ivy-Plus college are weakly positive. A 2% higher admission rate would therefore translate to a 0.04 pp upward-biased estimate of the treatment effect on the predicted probability of reaching the top 1% – two orders of magnitude smaller than our actual estimate of the treatment effect below. These calculations suggest that the multiple-rater test is adequately powered to detect meaningful degrees of bias.

income groups shown in Figure 2), race/ethnicity indicators, home state indicators, gender, recruited athlete status, and legacy status. The inclusion of these additional controls does not change the gap in admissions rates at other Ivy-Plus colleges among accepted vs. rejected students on the waitlist. In contrast, the inclusion of additional controls reduces the gap in admissions rates between accepted and rejected applicants not placed on the waitlist, consistent with the larger differences in credentials between those applicants. The third column of Figure 7 show that we obtain similar results in a “non-advantaged” sample dropping legacies, athletes, and the top 1% who receive preferences in the admissions process. Under Assumption 1, these tests imply that the variation in admissions decisions between waitlisted candidates is due to idiosyncratic factors rather than differences in underlying student quality and is thus orthogonal to their potential outcomes.

Balance Tests. To further assess the validity of our design, we test whether the characteristics of applicants admitted vs. rejected from the waitlist are balanced, pooling all Ivy-Plus colleges in our sample. We begin with an omnibus test of balance on the characteristics that matter for our post-college outcomes of interest. We regress the primary outcome we analyze – the predicted probability of reaching the top 1% based on firms and graduate school enrollment at ages 22-25 – on the following observable characteristics: a quintic in SAT/ACT scores, parent income dummies (the 13 bins shown in Figure 2), indicators for race and ethnicity, gender, home state, recruited athlete status, legacy status, fixed effects for academic and non-academic ratings, and college-by-cohort fixed effects. We then compare the predicted values from this regression among admitted vs. rejected students by regressing the predicted outcome on an indicator for admissions and fixed effects for the college to which students applied.³² The first row of Appendix Table 7 shows the predicted probability of reaching the top 1% in the rejected group and the admitted group (adding the coefficient from the regression to the rejected group mean). We find similar predicted values in the two groups, with a small, statistically insignificant ($p = 0.43$) difference of -0.1101 (relative to a standard deviation within the non-admitted group of 4.65).

To further probe balance and obtain insight into the factors that influence admissions from the waitlist, we next compare the observable characteristics of those admitted vs. rejected from the waitlist. We find balance on most of these variables, including student demographics, academic credentials, and a measure of high school quality, defined as the average predicted probability of reaching the top 1% based on the high school a student attends. However, children of alumni and those from the top 1% are significantly more likely to be admitted to Ivy-Plus colleges off the waitlist, consistent with prior evidence from case studies (Golden 2006). That is, the same factors identified above that lead to an admissions advantage for high-income applicants in general also lead to an admissions advantage from the waitlist. This imbalance turns out to not matter for potential outcomes, however, because legacy status and the other factors that lead to higher admissions rates for students from high-income families are uncorrelated with post-college outcomes, a result that we establish in Section 5 below. Nevertheless, to ensure that the imbalance related to parental

³²We estimate these and all subsequent regressions in this section in a dataset with one observation per student per Ivy-Plus college at which they were waitlisted, clustering standard errors by student to account for the fact that some students appear on multiple waitlists. We weight the regressions to obtain an average treatment effect that weights each Ivy-Plus college in our sample equally.

income and legacy status does not affect our conclusions, we replicate our main causal estimates excluding legacies, athletes, and students with parents in the top 1% and show that we obtain very similar results to those reported below (see Appendix Table 8).

Results. Figure 8 plots treatment effects of waitlist admissions on various outcomes. To construct these charts, we first estimate the treatment effect of attending an Ivy-Plus college using the estimator in (5). We estimate this treatment effect by regressing the outcome on an indicator for being admitted (along with fixed effects for the college to which the student applied and, in certain specifications, additional controls). We then divide the coefficient on the admission indicator by the probability of attendance conditional on admission to obtain a treatment-on-the-treated (TOT) estimate of the causal effect of attendance for those admitted from the waitlist. Finally, we plot two values: the observed mean for those rejected from the waitlist and the same mean plus the estimated treatment effect.

We begin by examining how admission to an Ivy-Plus college affects the probability of reaching the upper tail of the income distribution at age 33 – where we find the largest treatment effects – and then return to impacts on other moments of the income distribution below. The first pair of bars in Figure 8a shows that students admitted from the waitlist are 5 percentage points more likely to reach the top 1% at age 33 than those who are rejected ($p < 0.05$). Although these estimates are adequate to reject the null hypothesis that admission to any Ivy-Plus college has no effect on outcomes, they have three limitations: first, the estimated effect sizes are imprecise, as shown by the wide confidence interval in Figure 8a); second, the treatment effect magnitudes are difficult to interpret because the outside option of students who are rejected has not been pinned down; and third, this analysis only captures the monetary impacts of Ivy-Plus attendance. The rest of this section addresses these three limitations.

Increasing Precision Using Predicted Outcomes. The reason the estimate in Figure 8a is imprecise is that we observe outcomes at age 33 for relatively few cohorts in our sample. In principle, one could include younger cohorts to increase precision; however, individuals' incomes change sharply during their late twenties, especially for graduates of highly selective colleges, many of whom attend graduate school or undertake clerkships or internships that have relatively low wages in their twenties. Figure 9a demonstrates this pattern within our data by plotting the share of students in the top 1% of the income distribution (relative to others of the same age), separately for students accepted vs. rejected from the waitlist. In both groups, the fraction in the top 1% of the income distribution rises sharply between ages 25 and 33. At age 33, those admitted from the waitlist are approximately 5 pp more likely to be in the top 1%, consistent with the estimates in Figure 8a. The difference is near 0 at age 25 and grows steadily with age, indicating that those admitted to Ivy-Plus colleges are placed on a different wage trajectory relative to those who are rejected.

Because of these differences in wage trajectories, we cannot directly measure earnings impacts at earlier ages, where we larger sample sizes. Instead, we use a surrogate index approach and predict individuals' probabilities of reaching the top 1% at age 33 using their employers or graduate schools at ages 22-25, as described in Section 2. Under the surrogacy assumption (Athey et al. 2019) that early-career employment

histories capture the causal pathways through which Ivy-Plus attendance affects income at age 33, the treatment effect on the predicted outcome provides an unbiased estimate of the treatment effect on incomes at age 33.

We evaluate whether this prediction model captures the income dynamics in Figure 9a in Appendix Figure 23a by dividing the sample into quintiles of predicted top 1% probability and plotting actual incomes by age for each group. As expected, mean incomes across the quintiles are bunched together at age 25 but fan out dramatically over time. By age 33, average income in the top quintile of firms is nearly \$1.5 million, compared to \$550k and \$350k in the 4th and 3rd quintiles respectively. Panel B shows that there is a strong, monotonic relationship between actual income and predicted top 1% income at age 33, confirming that the prediction model based on early-career employers predicts upper tail success at later ages and supporting the key surrogacy assumption.³³

Figure 8b shows that waitlist admits have a 2.75 percentage point higher predicted probability of reaching the top 1% at age 33 than those rejected from the waitlist based on their employers between ages 22-25. As expected due to the larger sample size, this estimate is considerably more precise, with a standard error of 0.7, allowing us to now reject the null hypothesis of no treatment effects with $p < 0.001$. Controlling for observable characteristics does not change these estimates significantly, consistent with the balance in characteristics between those admitted vs. rejected from the waitlists, as shown in the second set of bars in Figure 8b. Further limiting the sample to exclude legacy applicants, athletes, and students with parents in the top 1% – the attributes that are unbalanced in Appendix Table 7 and are associated with admissions advantages for high-income applicants – also does not change the estimates, as shown in the third set of bars in Figure 8b.

To benchmark the magnitude of these treatment effect estimates, we compare them to what one would predict based on observational estimates of college value-added (VA), constructed by regressing individuals' predicted probabilities of reaching the top 1% on fixed effects for the college they attended and a quintic in test scores, 13 parent income bins, and indicators for race, gender, and home state. We replicate the same specification as that used to estimate the treatment effects in Figure 8b (also reported in Column 1 of Appendix Table 8) using the observational VA of the college that students attend as the outcome instead of their observed outcomes. Students admitted from an Ivy-Plus waitlist attend colleges that are predicted to send an additional 3.2 percentage points of students to the top 1% based on the observational VA model (Column 5 of Appendix Table 8), similar to the point estimates obtained when examining actual outcomes.

Figure 8c replicates the analysis in Figure 8b using the predicted mean income rank (based on employers and graduate schools at ages 22-25) rather than the probability of reaching the top 1%. Admission to an Ivy-Plus college has no significant impact on predicted mean income rank. We similarly find no significant effect on actual mean income rank at age 33 (Appendix Table 8). One would expect that the increase in chances of reaching the top 1% should increase mean incomes in levels, even if has little impact on mean

³³The vast majority of the variance in the predicted top 1% share is driven by heterogeneity in outcomes within rather than between industries, indicating that our measure captures more than just sectoral choice and showing that firms are a better proxy for future incomes than industries (Appendix Table 16).

income ranks. Unfortunately, we lack adequate precision to obtain an informative estimate of the impacts of Ivy-Plus admission on mean income in levels using the waitlist design. We therefore reconcile our estimates on chances of reaching the top 1% with impacts on mean income and other moments of the distribution using higher-powered estimators when discussing quantile treatment effects in Section 4.5 below.

Heterogeneity in Outside Options. The magnitudes of the reduced-form estimates reported in Figure 8 are difficult to interpret because they depend on the outside options of students who are rejected from the waitlist. In particular, many students who are rejected from the waitlist at one Ivy-Plus college are admitted to other Ivy-Plus colleges, as shown in Figure 7. More generally, students rejected from Ivy-Plus colleges tend to attend colleges that have higher levels of value-added (based on observational estimates) relative to the flagship public institutions that are our target outside option (Appendix Figure 24a).

To identify the causal effects of Ivy-Plus attendance relative to the fixed outside option of attending a flagship public college (ϕ_{Ivy}), we first estimate how the causal effects of admission to Ivy-Plus colleges vary with students' outside options. For example, observational estimates indicate that students at Penn State – the flagship public university in Pennsylvania – have much lower chances of reaching the top 1% of the income distribution than those at UC-Berkeley (controlling for test scores). Consider two students who apply to an Ivy-Plus college, one of whom is from Pennsylvania and applies to Penn State as a fallback option, and another who is from California and applies to Berkeley as a fallback option. Is the causal effect of admission to an Ivy-Plus college larger for the student who has Penn State as a fallback compared to UC-Berkeley?

To operationalize this examination of heterogeneity in treatment effects by the strength of outside options, we classify applicants to each Ivy-Plus college into groups based on their home state, parental income, and race. We estimate the quality of outside options that applicants in each of these groups have as the mean observational value-added (estimated using a regression of outcomes on college fixed effects, controlling for parental income, test scores, race, gender, and home state as above) among non-waitlisted rejected applicants in that group. We then estimate the treatment effect of being admitted vs. rejected from an Ivy-Plus college for students in groups with high vs. low value-added outside options.

This grouping instrument approach to estimating the effect of differences in outside options relies on the assumption that there is no essential heterogeneity in the causal effect of attending an Ivy-Plus college for students in different groups (as in Bleemer 2021a). For instance, if the return to attending an Ivy-Plus college were different for students from California vs. Pennsylvania, even holding fixed their fallback option, then our approach would not yield a consistent estimate of the effect of attending an Ivy-Plus college relative to an average flagship public institution. While we cannot directly test this assumption, we find little heterogeneity in treatment effects across other observable dimensions such as parental income and test scores (see Section 4.5 below), suggesting that this assumption is a reasonable approximation.

Figure 10a plots the treatment effect of being admitted from the waitlist on the share of students predicted to reach the top 1% (based on their age 22-25 employers) vs. the strength of their outside options, controlling for fixed effects for the Ivy-Plus college to which they applied. To construct this figure, we bin the outside

options measure described above into ventiles (20 equal-sized bins) and then plot the mean treatment effect from the waitlist design vs. the predicted value of the mean outside option within each of these bins (see Appendix J for details). There is a clear downward-sloping relationship between the treatment effects of admission and the strength of students' outside options. Students whose outside options are on average as good in terms of value-added as the Ivy-Plus colleges in our college-specific sample (on the far right side of the figure) gain very little from admission to one of those Ivy-Plus colleges (a treatment effect near 0). At the other end of the quality spectrum, students whose mean outside option is comparable to the value-added of the average flagship public institution have a $\phi_{Ivy} = 5.14$ percentage point (s.e. = 1.290) higher predicted chance of reaching the top 1%.

Identifying heterogeneous treatment effects by outside options requires that students admitted vs. rejected from the waitlist have comparable potential outcomes not just on average but also within each outside options subgroup. Figure 10b evaluates this assumption by replicating Figure 10a using predicted chances of reaching the top 1% based on pre-determined characteristics (estimated as in the balance test in Appendix Table 7) as the outcome variable. There is no relationship between the predicted outcomes of admitted students and the strength of their outside option: we find placebo treatment effect estimates close to 0 across the entire distribution, consistent with the balance test in Appendix Table 7. The fact that the actual outcomes plotted in Panel A diverge so sharply from the predicted outcomes in Panel B further supports the view that the differences in outcomes observed between those admitted vs. rejected from the waitlist reflect the causal effect of attending an Ivy-Plus college.

The slope of the relationship plotted in Figure 10a is -0.79, indicating that most of the variation in observational value-added is driven by differences in causal effects of colleges rather than selection. In Appendix Table 9, we evaluate the sensitivity of this estimate to alternative specifications for students' outside options, such as defining individuals' groups purely based on geographic area (commuting zone), using a jackknife approach to exclude a student's own observation when estimating her outside options, or excluding fixed effects for the colleges to which students apply so that differences between Ivy-Plus colleges are also used to identify the coefficient. Across a range of specifications (described in the notes to Appendix Table 9), we find estimates ranging from 0.69-0.93, and as a result, the implied causal impact of attending an Ivy-Plus college instead of a state flagship is robust to the measure used to predict a student's fallback option.

An alternative approach to estimating ϕ_{Ivy} that does not require estimating heterogeneous treatment effects by outside options is to multiply the reduced-form estimate obtained from the waitlist design (plotted in the figures above and reported in Appendix Table 8, Column 1) by the ratio of the difference in observational VA between the average Ivy-Plus and flagship public college and the difference in observational VA for those admitted vs. rejected from Ivy-Plus colleges (reported in Appendix Table 8, Column 5). This rescaling estimator extrapolates from the local difference in mean outcomes for waitlist admits vs. rejects to what one would observe if the outside option were the average flagship public based on differences in

observational VA. This approach yields a point estimate (reported in Column 1 of Table 4) of $\phi_{Ivy} = 5.01$ (s.e. = 1.31), nearly identical to that obtained from estimating heterogeneous treatment effects by outside options in Figure 10a. We use this less data-intensive estimator for ϕ_{Ivy} below because it yields more precise estimates, especially in smaller subgroups.

In Columns 5 and 6 of Table 4, we summarize the treatment effects by reporting the mean outcome for Ivy-Plus attendees and the implied mean outcome had those students attended average flagship public colleges instead by subtracting the waitlist design treatment effect reported in Column 1 of Table 4 from the observed Ivy-Plus means in Column 6 of Table 4. We estimate that attending an Ivy-Plus college instead of a highly selective state flagship increases a student's predicted chance of reaching the top 1% from 11.8% to 16.8%.

Non-Monetary Outcomes. Our analysis thus far has focused solely on monetary outcomes. As Figure 1 shows, however, Ivy-Plus colleges appear to have an even greater share in other non-monetary measures of upper-tail success, such as attending elite graduate schools or achieving positions of influence in public service. While we cannot directly measure all the outcomes in Figure 1 because we can only analyze outcomes at relatively young ages and because of the rarity of outcomes such as becoming a senator, we can examine treatment effects on other non-monetary outcomes that are likely to be predictors of such long-term success.

We begin by examining treatment effects on attending elite (highly ranked) graduate schools as defined in Section 2.³⁴ Figure 9b replicates the analysis of treatment effects by age in Figure 9a using elite graduate school attending instead of top 1% earnings as the outcome. We see a mirror image pattern, with larger treatment effects of approximately 3-4 pp between the ages of 25-28 – the peak ages of graduate school attendance – and then smaller treatment effects in the late 20s and early 30s, precisely when earnings impacts begin to appear (presumably as students have completed graduate and now earn high incomes). Figure 11a shows that the estimated treatment effects on elite graduate school attendance (at age 28) are insensitive to controls. They are also similar to what one would predict based on observational estimates of value-added (Appendix Table 8, Column 5). Using the rescaling estimator described above (rescaling using graduate school value-added rather than earnings value-added), we estimate that attending an Ivy-Plus college increases the chance of attending an elite graduate school at age 28 by 5.6 pp, from 6.1% to 11.7% (Table 4, Panel B). Consistent with our findings for monetary outcomes, the treatment effects are confined to measures of upper-tail success on the graduate school dimension as well: admission to an Ivy-Plus college has no significant impact on the probability of attending a non-elite graduate school (Table 4, Panel B).

Of course, attending an elite graduate school or working at a firm that channels many employees to the top 1% are only some of the many potential pathways to success and influence. To capture such pathways more broadly, we use a revealed preference approach, inferring how “elite” a firm is based on whether it

³⁴To assess robustness, we constructed an alternative definition of “elite” graduate schools using rankings data from U.S. News and World Report. We compiled average rankings across graduate programs in humanities, social sciences, natural and physical sciences, medicine, business, and law. We then ranked universities by the average value of the rankings across all categories and defined “elite” graduate schools as universities ranked in the top 25 on this measure. This alternative definition yields very similar results (Appendix Figure 25).

attracts many students from Ivy-Plus colleges. As discussed in Section 2, we define an “elite” firm as one that has a particularly high ratio of Ivy-Plus graduates relative to graduates of state flagship institutions (excluding the college that the student herself attended, to avoid bias from finite-sample noise and any mechanical effects of higher probabilities of working at certain firms, e.g. due to geographic proximity). Figure 11b shows the reduced-form impact of admission from the waitlist on the probability of working at an elite firm. Attending an Ivy-Plus college increases the chance that a student works at an elite firm by 9.18 pp. Applying the rescaling approach described above, we estimate that attending an Ivy-Plus college increases the probability of working at an elite firm by 17.0 pp, from 8.5% at highly selective state flagships to 25.5% at Ivy-Plus colleges.

Elite firms include many firms that are also high-paying as well as firms that are attractive for non-monetary reasons. As discussed in Section 2, we isolate the latter component by residualizing the ratio used to define elite firms with respect to the predicted top 1% measure that we use above. We then define “prestigious” firms as those that rank highly on this residual. Figure 11c shows that being admitted to an Ivy-Plus college significantly increases students’ chances of working at a prestigious firm after college, with a reduced-form treatment effect of 8.8 pp. These estimates are, as with other outcomes, insensitive to the inclusion of controls and similar to what one would predict based on observational value-added estimates.³⁵ The rescaling estimator implies that Ivy-Plus attendance increases the probability of working at a prestigious firm by 17.5 pp, from 7.2% at highly selective state flagships to 24.7% at Ivy-Plus colleges.

4.4 Estimates Based on Idiosyncratic Variation in Matriculation

We now present results from our second research design, which exploits idiosyncratic variation in matriculation conditional on admissions offers, following Dale and Krueger (2002) and Mountjoy and Hickman (2021).

We begin by regressing students’ predicted probability of reaching the top 1% on indicators for the college they attended and indicator variables for the portfolio of colleges to which they were admitted, replicating the baseline specification in Mountjoy and Hickman (2021). The y axis of Figure 12a reports these fixed effect estimates when estimating this model using the Ivy-Plus colleges and state flagship institutions in our college-specific analysis sample. We plot these estimates against observational estimates of value-added, constructed as above.

The observational VA estimates and the estimates that condition on admissions portfolios are strongly positively correlated, with a slope of 0.82. The point estimate for the Ivy-Plus colleges (pooled together to preserve confidentiality) implies that attending an Ivy-Plus college instead of the average flagship public

³⁵One may be concerned that our definitions of “elite” and “prestigious” firms overweight firms in areas of the country that are proximate to Ivy-Plus colleges, such as large cities in the Northeast, potentially leading to a mechanical treatment effect if students tend to take jobs near their colleges. We address this concern in two ways. First, we recalculate the definition excluding all firms located in New York City and Boston. Second, we stratify the classification of elite and prestigious firms by census region (e.g. pulling from the top of the list of firms until we have accounted for 25% of Ivy-Plus employment *in each region*, rather than overall. We find similar, statistically significant effects of Ivy-Plus attendance on elite and prestigious firm employment with both of these alternative definitions, indicating that the treatment effects are not driven simply by geographic effects (Appendix Figure 34).

college (whose VA is normalized to 0) increases a student's predicted chance of reaching the top 1% by approximately 5.14pp, similar to the estimate obtained from our waitlist admissions design.

In Figure 12b, we expand the sample of colleges we consider to include several other colleges for which we have admissions data: University of California colleges, California State colleges, and public colleges in Texas (the sample used by Mountjoy and Hickman 2021). We continue to find a strong relationship between observational estimates of college VA and estimates that condition on admissions portfolios, with the exception of colleges in Texas, where, consistent with the results of Mountjoy and Hickman, we find essentially no variation in outcomes conditional on admissions portfolios.³⁶ Most importantly for our purposes, Ivy-Plus colleges remain well above all the other colleges in terms of their causal effects, with an estimated impact relative to the average flagship public college of approximately 5 pp.

When we replicate this analysis using predicted mean income ranks instead of the probability of reaching the top 1%, we find much smaller differences between Ivy-Plus colleges and other institutions (Figure 12c). The estimates on mean ranks obtained from the matriculation design are very similar to those obtained from the waitlist design. We also find small, statistically insignificant effects of Ivy-Plus attendance on log earnings (Appendix Table 8), reconciling our findings with those of Dale and Krueger (2014). Furthermore, Dale and Krueger proxy for college quality by the average SAT scores of admitted students rather than estimating college fixed effects directly. Within the set of highly selective colleges Dale and Krueger consider, average test scores turn out to be weakly associated with post-college earnings (Chetty et al. 2017). Hence, it is not that these colleges have no impact on earnings, but rather that mean test scores are not highly predictive of value-added within a sample of highly selective colleges. Figure 12c also explains why regression-discontinuity (RD) studies (starting from Hoekstra 2009) have found larger causal effects of attending a more selective college than matriculation-based studies (starting from Dale and Krueger 2002): RD studies typically compare two-year and four-year colleges that have significantly different value-added on mean earnings, whereas matriculation-based studies (with the exception of Mountjoy and Hickman's analysis of Texas colleges discussed above) typically compare elite colleges that have relatively similar value-added on mean earnings.

Finally, replicating this design using the other non-monetary outcomes considered above, we find large positive effects of attending an Ivy-Plus college on the probability of attending an elite graduate school, working at an elite firm, and working at a prestigious firm (see Appendix Figure 27 and Table 4), with magnitudes similar to those obtained from our first research design.

4.5 Quantile Treatment Effects and Selection vs. Causal Effects

In this subsection, we compare estimates across our designs and then use our estimates to (i) characterize quantile treatment effects and (ii) analyze the fraction of observed variation in outcomes across colleges that

³⁶Mountjoy and Hickman focus on in-state applicants in Texas; we replicate their results restricting to that sample in Appendix Figure 26a. In-state applicants to public four-year schools in California, shown in Appendix Figure 26b, again show a different pattern, showing that Texas is unique in exhibiting small differences in outcomes when comparing matriculants to different colleges. Why colleges in Texas exhibit a different pattern is an interesting question that we defer to future work; what is clear is that the same design implies that Ivy-Plus colleges have large positive causal effects on upper-tail outcomes.

is due to selection vs. causal effects.

Table 4 summarizes our estimates of the treatment effects of attending an Ivy-Plus college instead of a flagship public college using our three estimators: the waitlist idiosyncratic admissions design, the matriculation design, and observational estimates of differences in outcomes conditional on test scores and parent income. We obtain similar estimates for all of these outcomes across all three estimators. We find highly significant ($p < 0.001$) treatment effects ranging from 5.0-6.6 pp for the predicted probability of reaching the top 1% across the three estimators. Treatment effects on the probability of being in the top quartile and on mean income ranks are much smaller. Similarly, we find significant impacts on the probability of attending an elite graduate school, but much smaller, statistically insignificant effects on attending a non-elite graduate school. Finally, we find large, positive effects (exceeding 13 pp) across all the estimators on the probability of working at an elite or prestigious firm. This similarity of the estimates between the two quasi-experimental research designs and the observational estimates – each of which relies on different assumptions – strengthens the view that attending an Ivy-Plus college significantly improves children’s long-term outcomes. It also suggests that the treatment effects of Ivy-Plus attendance (after accounting for outside options) are not highly heterogeneous across students on different margins of choice: those on the margin of being admitted, on the margin of choosing where to enroll conditional on admission, or for the average student attending different colleges.³⁷

Given the similarity of the estimates across estimators, we compare the income distribution of Ivy-Plus students to students from flagship publics with similar test scores (the estimator that has the most precision) to characterize the impacts of Ivy-Plus attendance on quantiles of the income distribution and on mean income. Appendix Figure 28b plots the ratio of the density of the individual income distribution at age 33 for students who attended Ivy-Plus vs. flagship public colleges, both unconditionally and reweighting on test scores, parent income, gender, and race. The fraction of non-working individuals (those with 0 individual income) is approximately the same (with a ratio of 1) in both groups, consistent with estimates using the waitlist design (Appendix Table 8). Ivy-Plus students are less likely to earn between the 15th and 95th percentile of the income distribution relative to peers at state flagship colleges. Ivy-Plus students are much more likely to reach the very top of the income distribution: they are 1.4 times as likely as state flagship students with comparable test scores and demographics to have incomes between the 99th and 99.5th percentiles, 2.2 times more likely to have income between the 99.9-99.99 percentiles, and nearly 4 times more likely to have incomes above the 99.99th percentile (where average incomes are \$13.1 million). These differences arise because a significant number of Ivy-Plus students have exceptionally high incomes

³⁷Consistent with the similar treatment effects across different margins, we find no significant evidence of treatment effect heterogeneity across observable subgroups (e.g., by parent income, legacy status, etc.), although these tests for heterogeneity are not adequately powered to reject meaningful differences in outcomes across subgroups (Appendix Figure 12d, Appendix Table 10). Most importantly, we find that attending an Ivy-Plus college has large positive treatment effects on upper-tail success even for lower-income and middle-class students in both the waitlist and matriculation designs. Note that students from high-income families who are rejected from Ivy-Plus colleges tend to attend higher-value added-colleges – perhaps because they apply more widely or live in areas with better fallback public options (Appendix Figure 24b). As a result, low- and middle-income applicants stand to gain more from Ivy-Plus admission than students from the top 1%, even though the treatment effect relative to a fixed outside option may not vary with parental income.

even at age 33 – 5% of Ivy-Plus students earn more than \$586,000 and 1% earn more than \$1.9 million (Appendix Figure 28a, Appendix Table 11) – whereas many fewer students from flagship publics and other highly selective private colleges reach those income levels.

The quantile treatment effects documented in Appendix Figure 28 are inconsistent with a model with constant treatment effects across the income distribution. Appendix Table 12 compares the share of Ivy-Plus and flagship public students with comparable demographics and test scores who reach the top 1% to what one would expect to see if the treatment effect of Ivy-Plus admission was log-constant across all percentiles of the income distribution. To estimate the log-constant treatment effect, we calculate 100 quantiles of the age 33 income distribution for Ivy-Plus students and flagship public students with comparable demographics and test scores, compute the log difference in incomes at each quantile, and take an unweighted mean across all 100 quantiles to arrive at a mean proportional treatment effect of 23%. The actual Ivy-Plus treatment effect on the share reaching the top 1% income at age 33 is 6 times larger than what one would observe with a constant 23% treatment effect across all quantiles, implying that the impact on top incomes shown in Appendix Figure 28 cannot be mechanically driven by a log constant (proportional) rightward shift in the income distribution. In this sense, Ivy-Plus attendance has a disproportionate effect on achieving upper-tail success. As a result of this shift in the upper tail, Ivy-Plus students have \$101,000 higher mean incomes than students with comparable demographics and test scores who attend state flagship colleges (Appendix Figure 28a).

Selection vs. Causal Effects. Figure 13 shows how much of the observed variation in outcomes between Ivy-Plus colleges and highly selective state flagship institutions is driven by causal effects vs. selection. For each outcome, we plot three estimates: the observed mean outcome at state flagships, the implied mean outcome had Ivy-Plus attendees attended state flagships instead (estimated by rescaling the waitlist design estimates, as in Column 5 of Table 4), and the observed mean outcome at Ivy-Plus colleges.³⁸ About 49% of the difference in the share who reach the top 1% and attend elite graduate programs between individuals who attended Ivy-Plus colleges vs. highly selective state flagships is due to the causal effect of Ivy-Plus colleges, with the remaining 51% driven by the fact that Ivy-Plus colleges select stronger students. The causal share of the difference is even larger for our measures of working at elite and prestigious firms.

In sum, although highly selective private colleges select students with unusually high potential, much of the difference in observed outcomes across colleges is due to treatment effects. Attending an Ivy-Plus college increases a student’s chance of reaching the top 1% by 49%, attending an elite graduate program by 92%, and the chances of working at a prestigious firm by 245%. The fact that treatment effects are largest for non-monetary outcomes echoes the finding in Figure 1 that Ivy-Plus colleges account for an even larger share of individuals in leadership positions as defined in non-monetary terms relative to those at the top of

³⁸We estimate the probability of having income in the top 1% at age 33 by multiplying the difference in the observational VA estimate at age 33 (8.4 pp) by the ratio of the waitlist design to observational VA estimate for predicted incomes based on the age 22-25 employers, which we are able to estimate with greater precision. This approach yields a smaller estimate than using our estimate of the actual top 1% treatment effect directly since that point estimate is larger than the estimated effect on observational VA (Figure 8a). We estimate mean income ranks at age 33 using an analogous approach.

the income distribution.

5 Differences in Outcomes by Admissions Credentials

In this section, we analyze whether the credentials underlying the high-income admissions advantage (legacy, athlete status, and high non-academic ratings) and other factors (e.g., test scores and academic ratings) are associated with better post-college outcomes. These outcome-based tests provide an input into evaluating the merits of weighing these credentials in the admissions process and shed light on whether colleges face tradeoffs between admitting more students from middle-class families and class quality (as judged by students' post-college outcomes).

Our goal is to identify the average difference in potential outcomes (e.g., probability of reaching the top 1%) for Ivy-Plus students with different credentials (e.g., legacy vs. non-legacy students). We describe the intuition underlying our approach to estimating this difference here; see Appendix K for formal derivations.

We begin by regressing post-college outcomes Y_i on four binary indicators of academic credentials: indicators for legacy status, being a recruited athlete, having high non-academic ratings, and having high academic ratings. We estimate these regressions in the sample of all waitlisted applicants and admitted students, excluding rejected applicants not placed on the waitlist (who are not close to the margin of admission). The solid bars in Figure 14a plot the coefficients from such regressions using the predicted probability of reaching the top 1% as the outcome Y . Ivy-Plus applicants' chances of reaching the top 1% after college are essentially unrelated to legacy status or their non-academic ratings. Recruited athletes are 3.1 pp more likely to reach the top 1% (relative to a baseline rate of 12.4% among non-athlete, non-legacy applicants with low academic and non-academic ratings). Those with high academic ratings are 5.8 pp more likely to reach the top 1%.

The raw comparisons of Y_i in the solid bars combine differences in latent earnings potential with the fact that applicants with certain credentials are more likely to be admitted to Ivy-Plus colleges, which channel more students to the top 1%, as shown above. The second (cross-hatched) set of bars in Figure 14a show how much of the difference in outcomes is due to differences in the quality of colleges by regressing the value-added $\hat{\phi}_{j_D(i)}$ of the colleges that students actually attend on the same four indicators. We estimate college value-added by regressing the outcome Y_i on college fixed effects and controls for test scores, parental income, and demographics and adjusting for residual unobserved selection by multiplying the raw fixed effect estimates by the ratio of the causal effect estimates from our waitlist design to the corresponding observational estimate for outcome Y (see Appendix K for details). The estimates in Figure 14a confirm that recruited athletes, legacies, and students with higher academic and non-academic ratings attend higher value-added colleges, i.e. colleges that increase their students' chances of reaching the top 1%.

Finally, in Figure 14b, we plot the difference between the two sets of bars plotted in Figure 14a, which reflects the difference in students' potential outcomes holding fixed the quality of the college they attend (Y_i^{Ivy}). After adjusting for differences in college quality, athletic recruitment and non-academic ratings

have no significant association with students' predicted chances of reaching the top 1%.³⁹ Legacy status is negatively associated with children's chances of reaching the top 1%. By contrast, having a high (above-median) academic rating increases one's chances of reaching the top 1% by 4.8 pp (39%), a magnitude similar to the causal effect of attending an Ivy-Plus college relative to a state flagship college. These findings are robust to including a wide range of controls from our admissions model (Appendix Table 13).

Figures 14c and 14d replicate Figure 14b using indicators for attending an elite graduate school (measured at age 25 to maximize precision) and working at a prestigious firm as outcomes. We again find no association between legacy status and non-academic ratings with these outcomes. Recruited athletes are substantially less likely to attend elite graduate schools and work at prestigious firms than their peers. Students with high academic ratings are substantially more likely to achieve success on these non-monetary outcomes, with a 6.6 pp higher chance of attending an elite graduate school (relative to a baseline rate of 8.4%) and a 6.9 pp higher chance of working at a prestigious firm (relative to a baseline rate of 21.9%).

Much of the predictive power from academic ratings stems from the predictive power of standardized test scores. Among applicants to Ivy-Plus colleges, students with higher SAT/ACT scores have substantially better post-college outcomes, adjusting for the quality of colleges they attend (Appendix Figures 29 and 30). SAT/ACT scores remain strongly predictive of outcomes even conditional on high school grade point averages (Appendix Figures 29 and 30), whereas GPAs are essentially unrelated to outcomes.⁴⁰ Test scores remain highly predictive of outcomes even within race-gender-parent-income cells and with high school fixed effects; in contrast, high school GPA is essentially unrelated to outcomes unless one includes high school fixed effects, perhaps reflecting differences in grading rubrics or peer quality across high schools (Appendix Table 13).

These findings show that standardized tests contain substantial information about student potential despite the biases that may arise from disparities in test preparation. Still, higher academic ratings predict better post-college outcomes even conditional on standardized test scores (Appendix Table 14). Admissions processes that take into account the strength of a student's coursework and other qualifications help identify student potential above and beyond standardized measures when focused on academic assessment. In contrast, the non-academic factors that are responsible for the higher admissions rates of students from high-income families do not predict (or, if anything, negatively predict) the measures of post-college success we consider.⁴¹

³⁹This two-step estimator avoids conditioning on the endogenous outcome of college attendance, which could yield biased estimates; in practice, however, we find very similar results when comparing outcomes among Ivy-Plus attendees with different application credentials (Appendix L).

⁴⁰We caution that our analysis applies only to Ivy-Plus applicants and the predictive power of test scores and GPAs may differ in other settings. For example, Rothstein (2004) finds that HS GPAs predict first-year grades better than SAT scores for students at University of California colleges. One potential explanation for the difference in results is that the predictive power of high school GPAs is weaker in the pool of Ivy-Plus applicants, who come from schools across the nation and may have GPAs closer to the maximum.

⁴¹This conclusion may appear to be inconsistent with evidence that Ivy-Plus attendees from high-income families have greater chances of reaching the top 1% (Chetty et al. 2020 and Michelman, Price, and Zimmerman 2021, replicated here in Appendix Figure 31). Appendix Figure 32a shows that children from higher-income families are more likely to work in higher-paying business sectors (finance, consulting, or technology) and less likely to work in lower-paying non-profit or public sector positions (health, education, government, or civic organizations). The probability of attending an elite graduate school or working at a prestigious firm does not vary with parent income. These results suggest that the drivers of the cross-sectional differences in

6 Predicted Impacts of Changes in Admissions Practices

In this section, we combine the results from the preceding analyses to answer our motivating question: how would changes in admissions practices at Ivy-Plus colleges affect the diversity of society’s leaders? We first analyze how changes in admissions policies that reduce the high-income admissions advantages identified above would affect the composition of the student body at Ivy-Plus colleges and their early-career outcomes. We then extrapolate to predict impacts of these changes on the socioeconomic backgrounds of society’s leaders.

6.1 Impacts on Student Bodies and Early-Career Outcomes

We make three key assumptions to predict the impacts of changes in admissions policies. First, we assume that students do not change their application patterns or aspects of their applications (e.g., investments in academic or non-academic qualifications) in response to the change in admissions practices. Second, we assume that students affected by the admissions policy change (i.e., those newly admitted or rejected) have matriculation rates that are the same as the average matriculation rates for currently admitted students with the same characteristics. Third, to predict how student outcomes would change in these counterfactual scenarios, we assume that colleges’ causal effects do not change with the composition of the student body.

Legacy Preferences. We begin by considering a policy that removes legacy preferences for all students. We exclude recruited athletes from this analysis since they are not admitted through the same process and return to them below.

We model the impacts of eliminating legacy preferences in two steps (see Appendix F for details). First, we take the estimated “legacy boost” from Figure 5 and proportionally de-admit a corresponding number of currently admitted legacy students, separately by parental income and test score to allow for the heterogeneity in the legacy advantage across subgroups shown in Figure 5b. For example, among students from families in the top 1% with test scores above 1500, legacy students are admitted to an Ivy-Plus college at roughly 4 times the rate as non-legacy applicants with comparable credentials. We therefore down-weight the number of legacy students in the admitted class who are from the top 1% and have test scores above 1500 by 75%.

The de-admission step releases 112 slots, which can now be filled by other students. We then refill the number of slots released by admitting students from the waitlist (as well as the pool of newly rejected legacies from the first step) in proportion to their predicted admissions probability from the non-legacy admissions model in Section 3.3.

Table 5a presents the impacts of this counterfactual admissions policy on parental income distributions. Eliminating legacy preferences and refilling the class as described above would reduce the fraction of students with parents in the top 1% from 15.8% in the actual data for Ivy-Plus colleges to 13.7%.

observed incomes by parental income among Ivy-Plus attendees are distinct from the forces driving the higher admissions rates of high-income students. Even though the average student from a high-income family earns more than students from lower income families (largely due to differences in career choice), the *marginal* student who is admitted due to legacy preferences, athletic recruiting, or non-academic ratings does not have better post-college outcomes.

Following the methodology in Section 5, we predict the impacts of this admissions policy change on students' outcomes by calculating the mean potential outcome (Y_i^{Ivy}) for the students who attend an Ivy-Plus college under the counterfactual admissions policy and comparing it to the mean observed outcome Y_i among actual Ivy-Plus attendees in our data. Eliminating legacy preferences increases the share of students predicted to reach the top 1% based on their employers at ages 22-25, the share of students working at prestigious firms (as defined in Section 2.3), and the share of students attending an elite graduate school. These predictions follow intuitively from the analysis in Section 5: legacy students are less likely to work at prestigious firms or attend elite graduate schools, and thus reducing the share of legacy admits improves average outcomes.

Non-Academic Ratings. Next, consider a policy that eliminates the admissions advantage that arises from the higher non-academic ratings enjoyed by students from high-income families, for instance by down-weighting the weight placed on non-academic accomplishments for high-income students. Similar to the “legacy boost” in the previous counterfactual, we estimate the “non-academic ratings boost” as the difference in admissions rates between students from families with incomes above the 80th percentile and those with similar academic credentials (measured both by test scores and academic ratings) from the middle class, allowing for heterogeneity by parent income and academic credentials. For example, among applicants with strong academic credentials (test scores above 1500 and high academic ratings), we estimate that students from families in the top 1% are 25% more likely to be admitted than they would if they had non-academic ratings comparable to those from the middle class (based on estimates from an admissions model analogous to that used in Section 3.3; see Appendix F for details). We down-weight the number of admitted students in proportion to this non-academic ratings boost and then refill the class in the same proportional manner as in the legacy counterfactual.

Eliminating the influence of higher non-academic ratings among students from high-income families on admissions would further reduce the fraction of students from the top 1%, from 13.7% to 11.1%. This change would also increase the fraction of students working at prestigious firms and attending elite graduate schools, although it would slightly lower the fraction of students predicted to reach the top 1% based on their employers at ages 22-25 because students from middle-income families are less likely to have earnings in the top 1% than those from high-income families (potentially because of the occupational choice effects discussed above).

Recruited Athletes. Finally, consider a policy that would remove the disproportionate representation of high-income students among recruited athletes, so that the distribution of parental income among recruited athletes matches the parental income distribution of students with test scores comparable to non-athlete Ivy-Plus college attendees. Such a policy would further reduce the overall share of Ivy-Plus students who come from the top 1% from 11.1% to 9.9%. While the share of students predicted to reach the top 1% based on their employers at ages 22-25 would increase only slightly – athletes are on average as financially successful as other students – the share of students attending elite graduate schools at age 28 or working at

prestigious firms at age 25 would increase sharply.

Together, these three changes in admissions practices would reduce the share of students from the top 1% at Ivy-Plus colleges by approximately 40%, from 15.8% to 9.9%. These changes would also reduce the share of students from families with incomes between the 95th and 99th percentiles (\$222-\$611K) by 2.9pp, as shown in Table 5a. The share of students from families in the bottom 60% of the parental income distribution would increase by 4.3 pp and the share from the bottom 95% would increase by 8.8 pp. Average student outcomes would not change or, if anything, improve along all three dimensions we consider.

Need-Affirmative Policies. An alternative approach to increasing the representation of students from low- and middle-income backgrounds is to simply offer them their own offsetting advantage in admissions, an approach sometimes termed “need-affirmative” admissions. Unlike the previous analyses – which involve de-admitting a specific subset of students and then refilling from the pool – this approach requires admitting a substantially new class. We therefore directly estimate the admissions rate of all students who were either admitted or on the waitlist at Ivy-Plus colleges (preserving all existing admissions preferences as we observe them in the data). We then proportionally increase admissions rates of students below the 95th percentile of the parent income distribution who have high academic ratings (motivated by the evidence in Section 5 that academic ratings are most predictive of post-college outcomes). See Appendix F for details.

We find that one can match the income shares produced by eliminating high-income admissions advantages with admissions rates that are 60% higher for students between the 60th and 95th percentiles than for students in the top 5% with comparable admissions credentials and 130% higher for students from the bottom 60% relative to those from the top 5% with comparable credentials.⁴² These admissions preferences for low- and middle-income students are about one-third as large as the preferences currently given to legacy students (which are approximately 300%, as shown in Figure 5c). Importantly, the additional students admitted under this need-affirmative counterfactual have *better* post-college early career outcomes than current Ivy-Plus attendees (row 5 of Table 5a). This is because the additional low- and middle-income students admitted under this policy have higher academic ratings, which as discussed above are associated with better post-college outcomes along the dimensions we analyze.

The preceding calculations apply to a single Ivy-Plus college changing its admissions practices by itself. Are there enough high-achieving low- and middle-income students who apply to Ivy-Plus colleges to implement such a policy across all Ivy-Plus colleges? Implementing such a policy across all 12 Ivy-Plus colleges would require increasing the total number of enrolled students with high academic ratings from the bottom 95% of the parent income distribution from 7,000 to 10,000 (250 per college). We estimate that there are 11,050 students who have high academic ratings who currently apply to at least one Ivy-Plus college each year.⁴³ Hence, there is likely an adequate supply of high-achieving, low-income students even among the

⁴²Row 5 of Table 5a shows that the reduction in students from the top 5% with the need-affirmative policy falls more heavily on students between the 95th and 99th percentiles than the top 1% relative to the counterfactuals that remove the three high-income admissions advantages. Intuitively, those three policies directly target the sources of the spike in admissions rates for students from the top 1%; absent need-affirmative policies that benefit all students from the bottom 99%, need-affirmative policies will not address the sharp differences in admissions rates between students within the top 5%.

⁴³We estimate this number by using the internal admissions records to calculate the share of students with a high academic

current applicant pool to implement a need-affirmative policy. The supply of such students also suggests that Ivy-Plus colleges could expand the size of incoming classes without compromising academic standards; even without changing the socioeconomic mix of students, this expansion would allow more students to benefit from the large causal effects of attendance with corresponding social benefits.

Going beyond changes in admissions policies, in Row 6 of Table 5a we consider an “income neutral” counterfactual in which Ivy-Plus attendance rates do not vary with parental income conditional on test scores. This counterfactual effectively augments that considered in Row 5 to equate not just admissions rates but also application and matriculation rates by parental income conditional on test scores. These additional changes would further reduce the top 1% income share at Ivy-Plus colleges to 7.2%.⁴⁴ Of course, one could increase the representation of children from lower-income families even further by increasing the preference given to lower-income students beyond that needed to achieve parity in admissions probabilities (conditional on credentials) across the income distribution. In row 7, we consider one such policy modeled by Chetty et al. (2020), which gives students from the bottom 20% of the income distribution an advantage in the college application and admissions process comparable in magnitude to the admissions preference currently given to legacy applicants. In this counterfactual, Ivy-Plus attendance rates for students from the bottom 20% equal those of students with 160 point higher SAT scores from the top 20%, along with smaller boosts for students from middle-income families. As shown in Chetty et al. (2020), such a “legacy-equivalent” need affirmative admissions policy would result in a parental income distribution of students at Ivy-Plus colleges that matches that of college students as a whole, increasing the share of students from the bottom 60% of the income distribution at Ivy-Plus colleges by 28.8 pp relative to current levels.

6.2 Impacts on Socioeconomic Backgrounds of Society’s Leaders

We now predict the impacts of the changes in admissions policies discussed above on the socioeconomic backgrounds of individuals holding the leadership positions discussed in Figure 1. To do so, we combine the preceding estimates of changes to the socioeconomic composition of the class with our estimates of the causal effects of Ivy-Plus attendance. Importantly, we assume that these causal effects do not vary with parent income, consistent with the evidence in Figure 12d. Under this assumption, it is straightforward to calculate the incremental number of leaders who come from a given parental income background using Bayes’ rule (see Appendix M for details).

We begin by examining monetary outcomes, in particular the backgrounds of individuals who reach the top 1% of the income distribution. To gauge magnitudes, consider an upper bound scenario in which *all* Ivy-Plus students come from families in the bottom 60% of the income distribution (an 84.3 pp increase from the 15.7% of students who come from the bottom 60% at present). Our estimates in Section 4 imply

rating at each test score level, and then we multiply this share by the total number of scores at each level above an SAT of 1400 or ACT of 31 from students in the bottom 95% of the parent income distribution. To calculate the total number of such students who apply to at least one Ivy-Plus school, we calculate the total number of applications across all colleges and divide by the average number of Ivy-Plus scores ends among students who sent a score to at least one Ivy-Plus college.

⁴⁴Predicting the impacts of this counterfactual on post-college outcomes would require predicting outcomes for students who do not currently apply to Ivy-Plus colleges; since our estimates of potential outcomes in Section 5 do not consider non-applicants, we cannot predict post-college outcomes for this or the next counterfactual.

that attending an Ivy-Plus college instead of the average state flagship increases a child's chances of reaching the top 1% by 6.4 pp. Changes in Ivy-Plus admissions could therefore create a maximum of an additional $84.3\% \times 6.4\% = 5.4$ percentage points of Ivy-Plus students who both come from the bottom 60% and reach the top 1% (and correspondingly fewer Ivy-Plus students from other backgrounds who reach the top 1%). Because only 0.47% of individuals attend Ivy-Plus colleges, however, even this extreme admissions change would increase the share of top 1% earners from low-income families by only $0.47\% \times 5.4\% / .01 = 2.53$ pp. The admissions reform that eliminates the three high-income admissions advantages discussed above – which increases the bottom 60% share at Ivy-Plus colleges by 4.3 pp – would increase the overall share of top 1% earners from low-income backgrounds by approximately 0.1 pp, as shown in row 1 of Table 5b. Intuitively, changes in Ivy-Plus admissions policies cannot have very large effects on monetary measures of upper-tail success because there are many paths to the upper tail of the income distribution that do not involve attending an Ivy-Plus college.

For non-monetary outcomes, a much larger share of those in leadership positions – e.g., those who win Nobel prizes or become Supreme Court justices – attend Ivy-Plus colleges (Figure 1), creating scope for larger effects of changes in Ivy-Plus admissions policies on these outcomes. As discussed above, we cannot directly estimate treatment effects on these leadership outcomes because they are rare and because our data are censored. Instead, we extrapolate from our estimated treatment effects on reaching the top 1% (for the Business outcomes listed in Panel A of Figure 1), attending an elite graduate school (for the Arts and Sciences outcomes in Panel B), or working at prestigious firms (for the Public Service outcomes in Panel C) to predict these impacts.

For example, our estimates in Figure 13 imply that 85% of the observational difference in rates of working at prestigious firms between students who attend Ivy-Plus vs. state flagship colleges is due to a causal treatment effect of Ivy-Plus attendance. The key assumption we make to predict impacts on public service leadership outcomes is that this 85% ratio is the same for leadership outcomes, i.e., 85% of the observational difference between the rates at which students from Ivy-Plus and flagship public colleges attain leadership outcomes is due to causal effects. Combined with the assumption of homogeneous treatment effects by parent income discussed above, we then use these predicted treatment effects and apply the same approach as that discussed above to predict how the socioeconomic backgrounds of those holding leadership positions would change (see Appendix M for details).

Ivy-Plus graduates account for 71% of recent Supreme Court justices (compared with 0% for graduates from flagship public schools). The increase of 4.3 pp in the fraction of students from the bottom 60% from eliminating high-income admissions advantages at Ivy-Plus colleges would increase the share of justices from families in the bottom 60% by $4.3 \times 0.85 \times 71\% = 2.6$ pp (Table 5b). Under our maintained assumption of constant treatment effects, the more aggressive “legacy-equivalent” need-affirmative reform (from Table 5a, Row 7) would increase the share by 17.5 pp – roughly an additional four justices from the bottom 60% over the past fifty years. The same reform would generate another 9 Nobel laureates (6.1 pp), 5.6 sitting senators

(5.6 pp), and 2.8 Treasury secretaries (12.6pp) from families in the bottom 60% (Table 5b).

These impacts reflect a scenario in which only the twelve Ivy-Plus colleges change their admissions practices, ignoring any potential changes in admissions practices at other colleges that may follow suit. Based on observational value-added estimates, we estimate that the total effects on socioeconomic diversity would be approximately 2.5 times larger if the next 60 highest-ranked private colleges (based on Barron's classifications) made similar changes to those considered above at the 12 Ivy-Plus institutions (see Appendix M for details).

7 Conclusion

This paper has established two main results regarding the determinants and consequences of attending Ivy-Plus colleges. First, attending an Ivy-Plus college instead of a flagship public college substantially increases students' chances of achieving upper-tail success both in terms of earnings and non-monetary outcomes. The magnitudes of the treatment effects are substantial: we estimate that attending an Ivy-Plus college instead of a flagship public increases mean incomes by \$101,000, equivalent to \$80.32 billion per year across all individuals who attended Ivy-Plus colleges. The spillover effects from these impacts on society are likely substantial: even setting aside potential impacts on innovation, public policy, etc., the incremental tax revenue from the treatment effect on income is \$24.10 billion per year (see Appendix N). These calculations do not take into account potential general equilibrium effects and other non-monetary impacts, but they illustrate the outsized impacts that a dozen highly-selective colleges in the U.S. have on society.

Second, Ivy-Plus colleges admit students from high-income (top 1%) families at substantially higher rates than students from middle-class families with comparable academic credentials. The high-income admissions advantage arises from admissions preferences given to children of alumni, to students from certain high schools that produce strong non-academic credentials, and to recruited athletes. These colleges therefore have the capacity to diversify society's leaders in terms of their socioeconomic backgrounds by changing their admissions policies.

Flagship public colleges also have a larger share of students from very high-income families than middle-class families, but the gap there is driven by disparities in application rates rather than admissions rates. At public colleges, interventions to increase application rates from qualified students are likely to be more impactful in expanding access than changes in admissions policies. More generally, increasing socioeconomic diversity going forward will likely require different approaches across colleges. To help colleges and researchers determine which parts of the pipeline one should focus on (applications vs. admissions or matriculation) at a given college, we have publicly released a college-level dataset on parental income distributions at each stage of the application process at www.opportunityinsights.org/data (see Appendix O for details on this dataset).

At the broadest level, our findings underscore the importance of going beyond financial support to expand access to high-quality higher education. Most of the colleges we analyzed in this study offered extensive

financial aid for lower-income applicants but still had large differences in attendance rates by parental income. Increasing economic mobility will likely require a combination of financial support (which may be a necessary condition for lower-income students to attend expensive colleges) and other policy changes, consistent with recent findings in other policy domains (e.g., Bergman et al. 2023 and Katz et al. 2022).

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Online Appendix

A Statistics on Colleges Attended by Society's Leaders

This appendix describes how we construct Figure 1, which presents calculations of the share of elite occupations held by Ivy-Plus college attendees. The first row shows that Ivy-Plus attendees are 0.8 percent of all college students. We construct this statistic using the long-term outcomes sample and dropping test-takers who didn't go to college, as described in Section 2.

Income. The first set of outcomes focuses on the fraction of individuals in various upper quantiles of the individual income distribution at age 33 who attended Ivy-Plus colleges. We measure income as total pre-tax individual earnings using data from tax records and college attendance using the 1098-T data, Pell Grant data, and colleges' own attendance records, as described in Section 2. We use data from BoardEx, a dataset covering executives and directors from roughly 17,400 firms, restricting to those employed between 2015 and 2024 to identify the educational histories of business leaders. Using this data, we identify the share of members of boards and committees who earned degrees from Ivy-Plus universities. We combine these educational histories with data from Databahn (2024) on the names of 2024 Fortune 500 CEO's. Using these data, we identify 416 CEOs of 413 Fortune 500 companies.

Arts and Sciences. We measure graduate school attendance as the proportion of individuals who attended Ivy-Plus colleges, among test-takers who attended graduate school by age 28 (as described in Section 2). We define elite graduate schools as all Ivy-Plus institutions plus UC Berkeley, UCLA, UCSF, University of Virginia, and the University of Michigan, since these schools consistently rank highly across graduate programs in medicine, business, science, law, and other fields. We obtain data on the undergraduate institutions of individuals who were granted MacArthur Fellowships between 1981 and 2014 from Conrad (2015). We obtain data on the educational histories of all Nobel laureates from 1980-2015 from Wai et al. (2024) and restrict to US-born laureates using a Wikidata query of Wikipedia on their birthplaces.

Public Service. We measure the proportion of U.S. senators from the 117th Congress who attended Ivy-Plus colleges by combining information from Congress.gov and Wikipedia. We obtain statistics on undergraduate attendance of New York Times journalists from Benton (2024). Data on U.S. Presidents from 1961-2023 who attended Ivy-Plus colleges were collected from publicly available biographical resources by referring to the list of U.S. Presidents from the Library of Congress webpage. We compute the fraction of Rhodes scholarship winners from 2014 to 2021 who attended Ivy-Plus colleges using information from the Rhodes Trust webpage. The share of secretaries of the Treasury who attended Ivy-Plus institutions includes all appointments starting from C. Douglas Dillon in 1961 to Janet Yellen in 2021. We obtain information on the undergraduate institutions of Treasury secretaries from Wikipedia. The fraction of Supreme Court justices who went to Ivy-Plus colleges includes all appointments starting from Thurgood Marshall in 1967 to Ketanji Brown Jackson in 2022. We obtain information on the undergraduate institutions of Supreme Court justices from Wikipedia and other publicly available biographical resources by referring to the list of

appointments from Supreme Court of the United States webpage.

We also replicate these statistics for attendees of highly selective private and flagship public colleges in Appendix Figure 1. The list of highly selective private and public colleges used in this figure can be found in Appendix Table 1. All the outcomes in Appendix Figure 1 use the same definition and come from the same data sources as Figure 1.

B Measuring College Attendance

This Appendix provides more detail on our definition of college attendance, as discussed in Section 2.2. The first definition is constructed using comingled data from tax records (1098-T forms) and the National Student Loan Data System (Pell Grant records), as described in Appendix B of Chetty et al. (2020). 1098-T forms are filed by all Title IV-accredited institutions of higher education in each calendar year for all tuition-paying students. To identify students who do not pay tuition and might not receive a 1098-T form, we use data from Pell Grant records.⁴⁵ In combination, these two data sources provide a near-comprehensive roster of domestic student college attendance at higher education institutions in the United States. For each student and each calendar year, we define a student as attending a college if she appears in either the 1098-T or Pell Grant data for that school and year. We then assign each student to one college by defining the college attended as the college a student is matched to in most years between the ages of 19 and 22. If multiple schools are matched to a student for the same number of years, we define a student's college as the first college she attends.

Our second method of measuring college attendance is to use colleges' own attendance records. In these data, we define college attendance using a college-provided indicator for whether a student matriculates to a given university as a first-year undergraduate student.

When both attendance measures are available, they are typically well aligned; moreover, the attendance measures based on federal data have a correlation of 0.99 with enrollment counts from IPEDS (Chetty et al., 2020, Appendix B).

C Predicting Application Rates Using Scoresend Data

Our data provide two sources of information on students' applications to colleges. First, we observe applications to colleges for which we have linked internal data in our college-specific sample. Second, we observe colleges to which students send their standardized test scores (up to 33 colleges, although in practice students rarely hit this limit). These scoresends serve as an indicator of where a student intends to apply. However, students may send their test scores to schools to which they do not ultimately apply, and thus scoresends provide an imperfect signal of true application.

⁴⁵Since we do not have Pell records after 2013, our approach could potentially underestimate college attendance rates for students receiving full financial aid in the most recent years of our sample. However, the vast majority of colleges submit 1098-T forms for all students irrespective of whether they make tuition payments or not. As a result, this missing data problem has little effect on our results: we find very similar estimates when excluding college-year cells where enrollment coverage may be incomplete.

To address this problem and predict true application rates from scoresends data, we estimate the fraction of scoresends that result in actual applications at the subset of colleges for which we have internal application data. Among those students who sent a score, we regress an indicator for a completed application on quintic polynomials of parental income, student SAT/ACT score, and distance between a student’s home zip code and the college the student applied to. The predicted values from this regression give the estimated fraction of scoresends from a given type of student that convert into completed applications, heterogeneously based on students’ characteristics. We then apply these estimated fractions to all scoresends in the data to form preliminary estimates of the total number of applications as a subset of the total scoresends to each school.

These preliminary estimates do not capture students who apply to a college without sending their test scores (since some colleges do not require standardized tests) or who send their scores in a manner that we do not capture in our data (an issue that can arise in the ACT data since we do not see scores sent after students take the test whose score we record). This leads us to undercount the true number of applications. To adjust for this issue, we calculate the ratio of the total number of applications reported in the Integrated Postsecondary Education Data System (IPEDS) to our preliminary estimate of the total number of applications separately for highly selective private and public colleges and multiply our preliminary estimates by these ratios.

We validate these predictions using a hold-out approach, verifying that the gradients they generate for application rates by parental income closely match the gradients of actual applications by parental income in colleges held out when estimating the prediction model.

D Predicting Income Trajectories Using Initial Employers

This appendix describes how we predict income ranks and likelihood that a student reaches the top 1% at age 33 based on early career outcomes observed between ages 22-25. Individuals’ income ranks (especially after having attended highly selective colleges) do not stabilize until their thirties (Chetty et al. 2020). We can observe incomes after age 30 for only a small share of our analysis sample; we therefore use predictions based on early-career trajectories to form surrogate indices for incomes at age 33 based on early-career outcomes, as in Athey et al. (2019).

We estimate the prediction model using students who attended colleges in selectivity tiers 1-4 (Ivy-Plus, Other Elite, Highly Selective Public, and Highly Selective Private, totaling 176 colleges; see Chetty et al. 2020). For each student, and in each tax year, we first identify whether the student attended a graduate program based on Form 1098-T; for students with multiple 1098-T forms indicating graduate attendance, we select first full-time over part-time graduate schools. For students that have no graduate school attendance, we identify their firm as the W-2 payer from which students receive the highest earnings. This procedure yields an EIN (or graduate school ID) for each student in each calendar year ages 22-25.

We estimate prediction models using the earlier cohorts in our sample (birth cohorts 1977-1988). We include fixed effects for EIN/graduate school at each age (so that there are four sets of fixed effects on the

right-hand-side of the regression), estimating the coefficients separately for each age 33 outcome (income rank and an indicator for reaching top 1%). For birth cohorts within the 1977-1988 estimation sample, we estimate a cohort-specific set of coefficients that leaves out the own cohort; for birth cohorts 1989 and younger, we use a single set of coefficients estimated on the full estimation sample. For many EIN-age pairs, there are an insufficient number of students in the earlier cohort to construct a precise estimate; in such cases (defined as fewer than 7, not including those in a student's own cohort), we combine all graduate schools into one category and all firms into combined categories based on their 2-digit NAICS industry code, 9 bins of firm size (0, 1, 2-10, 11-50, 51-200, 201-500, 501-1000, 10001-5000 and >5000), and the quintile of the fraction of age 33 employees in the top 1% in that tax year. In some cases, even these larger combined bins do not suffice, in which case we further pool EINs using the firm size bins and the quintile of the fraction of age 33 employees in the top 1%. Finally, any remaining firms with fewer than 7 students (along with new firms without any employees in earlier cohorts) are combined into one category.

We compare this baseline prediction model to several other models in the first row of Appendix Table 15, which reports the out-of-sample R-squared for each prediction model. Column 1 uses just the student's firm or graduate school at age 25, the model used in an earlier version of this paper (Chetty, Deming, and Friedman 2023). Column 2 is our baseline specification, using firms and graduate schools from ages 22-25, which meaningfully increases the explanatory power of the model. Column 3 uses individuals' incomes at ages 22-25 additively to predict having income in the top 1% at age 33; for these students, income alone at these early ages poorly predicts incomes at age 33. Column 4 uses both an individuals' firms or graduate school as well as their incomes at ages 22-25; adding incomes to the model in Column 2 does not improve the predictive power. For each of these models, we also present an estimate of the causal effect of admission (comparable to the "Raw Means" estimate in Figure 8b); other than in Column 3, where the income-only model has very little predictive power, the estimates are substantively similar and not statistically different from that using our baseline model in Column 2.

We evaluate the quality of the baseline prediction model (from Column 2) in capturing income trajectories in Appendix Figure 23. Panel A plots average incomes from age 25 through age 33 for students in various quantile ranges of the predicted probability of reaching the top 1%. Small differences in average incomes at age 25 expand substantially through age 33, by which point students in the top 1% of the prediction (top series of dots) earn an average of nearly \$1.5 million, well above those with lower predicted values. Panel B presents a binned scatter plot of average incomes at age 33 for each ventile of the distribution of the prediction. There is a clear upward slope across the entire distribution, showing that the prediction has signal throughout; average incomes for those in the top four ventiles (top 20%) of predicted values are greater than the top 1% cutoff for age 33 income.

To shed further light on the nature of the variation generated by our preferred prediction model, Appendix Table 16 decomposes the variance in predicted top 1% income probability by industry.⁴⁶ Panel A shows that

⁴⁶We obtain six-digit North American Industry Classification System (NAICS) codes for firms from the IRS Business Returns Transaction File. Each firm is assigned the NAICS code that appears most frequently on its five most recently filed business

whether a student is employed in the finance industry (NAICS 52) accounts for just 16.7% of the variation. Another 15.0% of the variance can be accounted for by variation among firms within the finance industry, while the majority of variance comes from differences among firms outside the finance industry. Panel B presents similar results with respect to the combined finance, consulting (NAICS 54) or technology (NAICS 51) industries; the majority (56.8%) of the variation in our prediction comes among firms within these three sectors. Panel C shows that the explanatory power in our model is roughly evenly distributed among firms in these three sectors. Panel D shows that relatively little variation comes from students working in either the non-profit (NAICS 61 or 62) or the public (NAICS 92) sectors.

E Defining Elite and Prestigious Firms

In this appendix, we describe how we construct our “elite” and “prestigious” firm definitions. We begin with the list of firms (corresponding to EINs in W-2s), firm names, North American Industry Classification System (NAICS) codes, and flags for government and nonprofit firms from the IRS Business Returns Transaction Files metadata. Firms are identified using their names. In cases where the same firm appears multiple times under similar names, we pool the firms together by eliminating common qualifiers (e.g. LLC, Corporation, etc.).

We first calculate the share of all Ivy-Plus attendees in the 1979 to 1996 birth cohorts that work at each firm when they are age 25. We remove the attendee’s own college from the calculation of the firm-level shares. When students do not have firms at age 25, we fill them in using age 26, and then age 24. We then calculate the same share for the Highly Selective Public colleges. In instances where a firm employs zero Highly Selective Public attendees, we calculate the share as if there were one. We then compute a ratio of those shares to form a measure of disproportionate Ivy-Plus employment, restricting the sample to firms that employ at least 25 students and leaving the student’s own observation out of the share calculation altogether. We rank firms using this metric and define a firm as “elite” by pulling firms from the top of the list until we have accounted for 25% of Ivy-Plus attendee employment.

To measure high-status jobs that do not necessarily lead to high earnings, we regress each individual’s “elite” firm ratio (described above) on the predicted top 1% probability of the individual’s age 22-25 firms, which is described in more detail in Appendix D. We then calculate the residual from this regression and re-rank firms from highest to lowest according to the residual. Finally, we pull the firms with the highest residuals in order until we have accounted for 25% of Ivy-Plus employment, and label these firms with the highest residual ranks “prestigious” employers.

To validate our approach to identifying “prestigious” firms, we first identify law firms, hospitals, and universities using NAICS codes. Consulting firms cannot be reliably identified using NAICS codes. Instead, we identify a firm as a consulting firm if greater than 25% of the Ivy-Plus applicants it employs have

returns. Ties are broken in favor of more recently filed returns. We then assign each firm to an industry using the first two digits of its NAICS code. Using this approach, we are able to classify 86% of Ivy-Plus college attendees by the industry of their age 25 firms.

occupational titles on their W-2s that indicate they are consultants. We then compare publicly available lists of the law firms, hospitals, universities, and consulting firms to the set of firms considered “prestigious” under our approach. We find a high degree of overlap, as discussed in the main text.

To probe the robustness of our results, we also construct alternative “elite” and “prestigious” firm definitions using comparisons to a broader group of colleges (Appendix Figure 33) and using geographic restrictions (Appendix Figure 34).

F Decomposition and Counterfactual Methodology

In this appendix, we describe the methodology used in the decompositions in Table 2 and Appendix Table 6 and the policy counterfactuals in Table 5a.

F.1 Decomposition Analysis

The analysis for both Table 2 and Appendix Table 6 begins with a calculation of the total number of “extra” students from the top 1% of the income distribution implied by the higher attendance rates, conditional on test scores, in Figure 2b. We calculate counterfactual attendance assuming that each top 1% student would attend an Ivy-Plus school at the same rate as students with the same test score but from the 70th-80th percentiles of the parent income distribution. Formally, we calculate

$$\text{Counterfactual Attendance}_c = \sum_a N_{Top\ 1\%, a} \times \text{AttendRate}_{P70-80, ac}$$

from equation (1) in Section 3.1, for students with test score a and for college c . This results in overall attendance that is 64.2% lower than the current attendance of top 1% students. We then scale this difference to an incoming class of 1650 first-year students, which is the average among Ivy-Plus schools in our sample period. In our data, 15.8% of students (or 261 out of 1650) are from top 1% families; under the counterfactual attendance rate, which is 64.2% lower than actual attendance, this falls to 93 students. The difference is 168 students.

Athletes. We next decompose the 168 extra students into the part due to extra recruited athletes and that due to extra non-athletes. Using our internal data from certain Ivy-Plus colleges, we calculate that 13.5% of all students (222 out of 1650) are recruited athletes, and similarly that 15.8% of top 1% students (41 of 261) are recruited athletes. This implies that there are 1,428 total students who are not recruited athletes, of which 220 (15.4%) are from the top 1%. From this point forward, we analyze the athlete and non-athlete portions of the class separately.

As discussed in Section 3.2, it is difficult to quantify the contribution of each of the three parts of the pipeline for athletes given their unique admissions process. We therefore adjust the number of top 1% athletes so that the share of athletes from the top 1% is equal to the share of the student body from the top 1% under our equal attendance counterfactual. The result is that the number of athletes from the top 1% falls by 66%, from 41 to 14; thus, there are 27 extra top 1% students that result from athletic recruiting.

This also implies that 141 out of the total 220 non-athletes from the top 1% are extra and that there would be 79 non-athletes from the top 1% under the counterfactual.⁴⁷

Applications, Admissions, and Matriculation for Non-Athletes. We next decompose the 141 extra non-athletes from the top 1% into the portions due to application, admission, and matriculation. Since we seek an estimate for the average across all 12 Ivy-Plus schools, but do not have internal data from each (which is required to disaggregate admissions and matriculation), we proceed in four steps:

1. Using data on all Ivy-Plus schools, we calculate the proportional contributions of applications and the joint effect of admissions and matriculation (which we can directly estimate for the full set of Ivy-Plus schools).
2. Using internal data from certain Ivy-Plus colleges, we calculate the proportional contribution of admissions and matriculation, rescaling these components to match the overall estimated effect of admissions and matriculation from Step 1.
3. Using internal data from certain Ivy-Plus colleges, we calculate the proportional contribution of preferences for legacy students and of higher non-academic ratings for top 1% students, rescaling these components to match the overall estimated effect of admissions from Step 2.
4. We translate these proportional contributions into numbers of students, using different orderings for Table 2 and Appendix Table 6. We now describe each of these four steps in more detail.

Because the first four steps operate via proportional changes in the number of top 1% students, it is useful to estimate the contribution of each part of the pipeline in log-points, thus allowing for these components to add to the whole. Among non-athletes, the extra top 1% students represent 102 log-points (220 down to 79 top 1% students).

Step 1: We calculate the total contribution of application rates following a similar approach to overall attendance above; that is, we calculate the counterfactual number of attendees from top 1% families assuming that each top 1% student applies at the same rate as students with the same test score but from the 70th-80th percentiles of the parent income distribution, while keeping admissions and matriculation rates unchanged:

$$\text{Counterfactual Attendance}_c = \sum_a N_{Top\ 1\%,a} \times \text{ApplyRate}_{P70-80,ac} \times \text{AdmitRate}_{Top\ 1\%,ac} \times \text{MatricRate}_{Top\ 1\%,ac}$$

This results in 29.8% fewer attendees from the top 1%, or 35.4 log-points. This implies that the remaining 66.6 log-points result from differences in admissions and matriculation.

Step 2: We calculate the contribution of admissions within internal data from certain Ivy-Plus institutions using a variant of the equation above:

$$\text{Counterfactual Attendance}_c = \sum_a \text{Applicants}_{Top\ 1\%,a} \times \text{AdmitRate}_{P70-80,ac} \times \text{MatricRate}_{Top\ 1\%,ac}$$

⁴⁷The numbers we report here are rounded to the nearest whole number, but we use the exact numbers throughout the decomposition calculations.

That is, we calculate the counterfactual number of attending students from the top 1% assuming that each applicant from the top 1% is admitted at the same rate as students with the same test score but from the 70th-80th percentile of the parent income distribution among the pool of applicants, while keeping application rates unchanged. These calculations result in a 33.3% (40.4 log-points) reduction in the number of top 1% students from changing admissions rates; since the overall gap in combined admissions and matriculation at these certain Ivy-Plus schools is 41.4% (53.4 log-points), this implies that a further 12.2% (13.0 log-points) reduction in the number of top 1% students stems from changing matriculation rates.

We now reconcile the components of admissions and matriculation, calculated from a particular set of Ivy-Plus schools, to produce a consistent set of estimates for the full set of 12 Ivy-Plus schools. We estimated a total contribution of 66.6 log-points for the combination of admissions and matriculation at the full set of 12 Ivy-Plus colleges. Within our set of certain Ivy-Plus schools, we estimated a contribution of 53.5 log-points from these two factors. Among our set of certain Ivy-Plus schools, there is no clear pattern that one of these components accounts for more of the overall variation between schools than the other (proportional to their overall size). Thus, we scale our estimates of admissions and matriculation up proportionally to match the overall combined effect of 66.6 log-points; this implies that the contribution of admissions is 50.4 log-points, and the contribution of matriculation is 16.2 log-points, across all 12 Ivy-Plus schools.

Step 3: We calculate the contribution of preferences for legacy students and non-academic ratings to the overall admissions component. To calculate the contribution of legacy preferences, we use the same set of certain Ivy-Plus colleges as in Step 2. We make two adjustments to remove the two channels through which top 1% students gain from legacy preferences from Section 3.3. First, we reweight the distribution of students so that the fraction of legacy students among the top 1% matches the fraction among students from the 70th-80th percentiles. This reduces the fraction of top 1% students who are legacy students from 8.9% to 1.2% (as in Appendix Figure 11a). Second, we reduce the admissions advantage enjoyed by legacy students in the top 1% to match that enjoyed by students from the 70th-80th percentiles. To do so, we use the legacy and counterfactual non-legacy admissions rates from Appendix Figure 11b, which result from estimating a linear probability model of admissions for legacy and non-legacy students using characteristics observed in the admissions data. The variables in these models are: a quintic in test score, indicators for gender, seven categories for race and ethnicity (Hispanic/Latino students of all races, plus non-Hispanic/Latino students who are white, Black, American Indian/Alaskan Native, Native Hawaiian/Pacific Islander, Asian, and unknown), first-generation status, early applicant status, 13 parent income bins, the tuple of ratings from the admissions offices, high school GPA, when available, and high school fixed effects. For each student, we define the “legacy boost” as the difference between their predicted admissions rate from the legacy admissions model and that from the non-legacy admissions model (retaining the same high school fixed effect). We then calculate the difference between the average legacy boost for top 1% applicants (32.4pp) and that for 70th-80th percentile students with the same test scores (18.8pp), subtracting the difference (13.4pp) from the modeled admission rate of each top 1% legacy applicant. Using the new counterfactual admissions rates and

the counterfactual student weights, we recalculate the total number of attending top 1% students, which is 26.0% (30.1 log-points) fewer than the actual number of attending top 1% students.

Next, we calculate the contribution of removing the top 1% advantage in non-academic ratings. Due to the differences in the exact nature of ratings across schools, we calculate the contribution of non-academic ratings using data from one Ivy-Plus school (as described in Section 3.3). We begin from the counterfactual admissions rates and student weights described in the previous paragraph (which remove the top 1% additional advantage in legacy admissions) at that one school. Using the coefficients on the ratings tuple from the non-legacy admissions model, we calculate the difference between the average contribution of ratings to admissions for top 1% applicants as compared with that for 70th-80th percentile students with similar academic ratings and test scores, where we group students based on their academic rating (pooling certain rare ratings with nearby ones) and on whether their test score is equal to or greater than 1500 (SAT) or 34 (ACT). We then subtract this difference from the admissions rate of all top 1% students (including legacies) and recalculate the total number of attending students from the top 1%, keeping the model-predicted matriculation rates unchanged. Relative to the number at the end of the legacy counterfactual, removing the top 1% advantage in non-academic ratings results in 24.0% (27.5 log-points) lower top 1% attendance.

We now reconcile these estimates with the overall contribution of 50.4 log-points for non-athletic admissions from Step 2. We calculated the contribution of legacy preferences from each of our certain Ivy-Plus colleges, but we calculated the contribution of non-academic ratings from just one college. At the one college in which we calculated the contribution of non-academic ratings, the sum of the two contributions (in log-points) is just 2.2 log-points different from the entire contribution of admissions. We thus conclude that these two components together can account for the full admissions differential. Further, while the total admissions differential differs across our set of certain Ivy-Plus colleges, the contribution of legacy preferences is relatively similar across schools. We thus make the assumption that the contribution of non-academic ratings is equal to the difference between the full admissions contribution and the contribution of legacy preferences at each school. This implies that, within our set of certain Ivy-Plus schools, the total contribution of non-academic ratings is 20.3 log-points (50.4 log-points from all of admissions minus 30.1 log-points from legacy preferences).

Step 4: In this final step, we convert our proportional estimates into numbers of students out of a first-year class of 1650. Recall that the total number of top 1% students who are not recruited athletes falls from 220 in the actual class to 79 at the counterfactual attendance rates. Table 2 and Appendix Table 6 present two different approaches to decomposing the 141 extra top 1% students into the respective components.

In Table 2, we decompose the 141 extra top 1% students by stacking the components in an order reflecting one way in which schools might prioritize these changes, given the results in this paper. We first apply the changes in admissions; a 50.4 log-point (i.e., 40%) reduction in the number of top 1% students represents a reduction of 87 top 1% students, leaving 133 students (and 54 extra students) remaining from the top 1%. Within this 87, we apply the legacy component (30 log-points) first, accounting for 52 students, followed

by the non-academic ratings component (20 log-points), accounting for the remaining 35 students. We next apply the matriculation component; a 16.2 log-point (i.e., 15%) reduction leaves 113 students (and 34 extra students) remaining from the top 1%. Finally, we apply the application component; a 35.4 log-point (i.e., 30%) reduction accounts for the remaining 34 extra students from the top 1%.

In Appendix Table 6, we instead apply the components proportionally based on their size in log-points. This abstracts from the order in which schools might institute these changes. Since the application component represents 35% of the total effect in log-points (35.4 out of 102 log-points), we assign 35% of the overall reduction in non-athlete top 1% students (141) to applications, for a total of 49 students. We use a similar approach for the other components.

F.2 Policy Counterfactuals

In Table 5a, we analyze the consequences of three different admissions policy counterfactuals. We perform this analysis on the set of Ivy-Plus colleges in our college-specific sample, and we then scale the intermediate policy counterfactual estimates to match the attendance gap observed across all 12 Ivy-Plus institutions as described below.

Removing Legacy Preferences. The second row of Table 5a considers a counterfactual in which colleges eliminate existing preferences for legacy applicants. We implement this counterfactual in two steps: we first de-admit certain students who are not athletic recruits as a result of removing the preference for legacy applicants, and then we refill the class from applicants who are not athletic recruits and who were either rejected off the waitlist in reality or who were admitted in reality and de-admitted in the first step. We keep the set of attending students who are recruited athletes the same.

In the first step, we de-admit a share of the legacy students that were currently admitted. To do so, we calculate the predicted admissions rate for each legacy student using the legacy and non-legacy linear probability models, as described earlier in this Appendix; for each student i , denote these predicted admissions rates p_i^L and p_i^{NL} , respectively. Within each parent income bin p and test score range s (above/below SAT 1500 or ACT 34) cell, we then calculate the ratio of the average non-legacy admissions rate (p_{ps}^{NL}) to the average legacy admissions rate (p_{ps}^L). We then define the intermediate counterfactual admissions rate (i.e., the admissions weight after the de-admission step but before the re-admission step) for each admitted legacy student as

$$\tilde{p}_{ps} = \frac{p_{ps}^{NL}}{p_{ps}^L}$$

Intuitively, if legacy students from a given test score bucket and parent income group have predicted admissions rates that are three times higher than their counterfactual non-legacy admission rate, then we probabilistically de-admit two out of every three such students who are currently admitted. Admitted students who are either recruited athletes or not legacies retain $\tilde{p} = 1$, while students who were not admitted retain $\tilde{p} = 0$. Define p_i^M as an indicator for whether currently admitted students chose to matriculate; then

the total number of students remaining in the matriculating class after this de-admission step is

$$\tilde{N} = \sum_i \tilde{p}_i * p_i^M$$

In the second step, we admit additional students to increase the size of the class by $N^{miss} = N - \tilde{N}$ additional students, back to the original level. We admit students in this step from two pools: those students placed on the waitlist but never admitted in the data, and those students de-admitted in Step 1. We assume that students in the latter group would matriculate or not if re-admitted as they did in the data; for students in the former group, we model each student's matriculation rate using the same linear probability model as for admissions (estimated separately on students admitted in the early and regular admissions rounds) and predict matriculation rates. This process assumes that students newly admitted to the class in the counterfactual would have the same matriculation rate as do observably similar students who were actually admitted in the data, i.e. that matriculation rates for marginal students is the same as that of the average admitted student conditional on observables. For these students, define p_i^M as student i 's predicted matriculation rate.

We now admit students from these two groups using the predicted admissions probabilities from the non-legacy model (p_i^{NL}). In order to fill N^{miss} spots, we must re-admit students with probability

$$p_i^R = p_i^{NL} * \frac{N^{miss}}{\sum_i ((1 - \tilde{p}_i) * p_i^{NL} * P_i^M)}$$

where the denominator of the fraction corrects for the overall size of the pool for re-admissions. The final weight for each student i after the counterfactual policy of removing legacy preferences is

$$p_i^{cf} = p_i^M * (\tilde{p}_i + (1 - \tilde{p}_i) * p_i^R)$$

Note that this expression simplifies to just p_i^M for non-legacy students who were originally admitted (since $\tilde{p}_i = 1$) and to $p_i^M * p_i^R$ for students not originally admitted but on the waitlist (since $\tilde{p}_i = 0$). With these new weights, we can calculate class characteristics as the average of characteristic X_i among all applicants either initially accepted or placed on the waitlist using weights p_i^{cf} .

In practice, changes in legacy preferences may also affect which students are placed on the waitlist. In the full model from which we generate results in Table 5a, we therefore implement an initial de-waitlisting and re-waitlisting step before admitting students from the waitlist in the previous calculations. Formally, this generates a probability of being on the waitlist p_i^W for all students not accepted in the data, where $p_i^W = 1$ for non-legacy students actually on the waitlist and not accepted in the data. With this additional weighting, students are re-admitted with probability

$$p_i^R = p_i^{NL} * \frac{N^{miss}}{\sum_i ((1 - \tilde{p}_i) * p_i^W * p_i^{NL} * P_i^M)}$$

and receive final weight of

$$p_i^{cf} = p_i^M * (\tilde{p}_i + (1 - \tilde{p}_i) * p_i^W * p_i^R).$$

Removing the Influence of Privilege on Non-Academic Ratings. Starting from the end of the previous counterfactual (i.e. having already removed preferences for legacy students), the third row considers a counterfactual in which we remove the advantage in non-academic ratings enjoyed by students above the 70th-80th parent-income percentiles. We implement the non-academic ratings counterfactual using the same two-step process as in the legacy preferences counterfactual, where now we calculate the rates at which students are de-admitted (\tilde{p} above) and re-admitted (p^R) based on the influence of non-academic ratings. To highlight this parallel, we denote all weights in this section with q (so that for instance \tilde{q} is the intermediate admissions weight after the de-admission step, similar to \tilde{p} before), while we maintain p as the predicted admissions probability from various admissions models. Note that we now refer to “admitted” students as those probabilistically admitted after removing legacy preferences above, and similarly those on the waitlist.

In the first step, we de-admit a share of non-athlete admitted students. To do so, we use the estimates of the coefficients on indicators for each ratings-tuple in the admissions model; denote the coefficient for each rating-tuple r as ϕ_r and so the relevant coefficient for each individual i as $\phi_{r(i)}$ based on that individual’s rating. Note that all individuals with the same ratings-tuple share the coefficient $\phi_{r(i)}$ (since we do not interact the ratings-tuple fixed effect with any other variables in the admissions model). Within each test score range s (above / below SAT 1500 or ACT 34) and academic rating cell d , and for each parent income bin p above the 70th-80th percentiles, we then calculate the difference $\Delta\phi_{sdp} = \bar{\phi}_{sdp} - \bar{\phi}_{sd,P70-80}$, where $\bar{\phi}_{sdp}$ is the average of $\phi_{r(i)}$ for students with test score s , academic rating cell d , and parent income bin p ; intuitively, this difference $\Delta\phi_{sdp}$ is the admissions advantage enjoyed by students from higher income groups from non-academic ratings, as compared with students with similar test scores and academic ratings but from the 70th-80th percentiles. Defining $p_i^{NLNR} = p_i^{NL} - \Delta\phi_{sdp(i)}$ as the predicted admissions rate after removing the high-income privilege in non-academic ratings, we then define the intermediate counterfactual admissions rate for each student above the 80th percentile as

$$\tilde{q}_i = \frac{p_i^{NLNR}}{p_i^{NL}} * \frac{p_i^{cf}}{p_i^M}$$

Intuitively, if a certain group of students has an admissions rate that is only 80% as high after removing the inflated non-academic ratings, then we probabilistically de-admit one out of every five such admitted students. Because the non-academic rating counterfactual is implemented on top of the legacy counterfactual, we must apply the de-admission rate to the relative prevalence of such students in the admitted class at the end of the previous counterfactual ($\frac{p_i^{cf}}{p_i^M}$). Using the same matriculation rates p_i^M as in the legacy counterfactual, the total number of students remaining in the matriculating class after this de-admission step is

$$\tilde{M} = \sum_i \tilde{q}_{i,psd} * p_i^M$$

In the second step, as in the legacy counterfactual, we admit additional students to increase the size of the class by $M^{miss} = N - \tilde{M}$ additional students, back to the original level. We draw again from the pool of waitlist rejects (after a similar de-waitlisting and re-waitlisting step as above) and those de-admitted in the first step, and we use the same matriculation rate p_i^M for each student as in the legacy counterfactual. To

fill M^{miss} spots, we re-admit students with probability

$$q_i^R = p_i^{NLNR} * \frac{M^{miss}}{\sum_i ((1 - \tilde{q}_i) * q_i^W * p_i^{NLNR} * p_i^M)}$$

The final weight for each student i after the counterfactual policy of removing the influence of parent income on non-academic ratings is

$$q_i^{cf} = p_i^M * (\tilde{q}_i + (1 - \tilde{q}_i) * q_i^W * q_i^R)$$

Adjusting for the High-Income Advantage in Athletic Recruitment. Starting from the end of the previous counterfactual (i.e. having already removed preferences for legacy students and advantages for high-income applicants due to ratings), the fourth row considers a counterfactual in which we remove the advantage enjoyed by recruited athletes in the admissions pipeline. To do this, we model the limiting case in which the characteristics of the recruited athletes in the class match those of the non-athletes. In practice, we de-admit all recruited athletes from the class and then proportionally scale up the admission rates of the non-athlete admits to refill the class. If there are N_A recruited athletes in the class, we de-admit the N_A recruited athletes, and calculate the new weight for each student i as

$$q_i^{cf} * \frac{\sum_i q_i^{cf}}{\sum_i q_i^{cf} - N_A}$$

Need-Affirmative Admissions. Separately from the previous three stacked counterfactuals, the fifth row considers a counterfactual in which legacy advantages, athlete preferences, and the income gradient in non-academic ratings remain in place, but in which preference is given to low- and middle-income students with high academic ratings. We consider non-recruited athlete applicants who were either admitted or waitlisted, and we begin with the admissions rates for legacy and non-legacy applicants estimated in the admissions model above. We then increase the estimated admissions rates for each applicant with parent income below the 60th percentile with a high academic rating by a factor of F_1 , and the estimated admissions rates for each applicant with parent income between the 60th and 95th percentile with a high academic rating by a factor of F_2 . Admissions rates for student i with parent income percentile p in this counterfactual are

$$\tilde{v}_i = \begin{cases} F_1 * v_i & \text{if } p(i) < 60 \text{ and } d = HIGH \\ F_2 * v_i & \text{if } 60 \leq p(i) < 95 \text{ and } d = HIGH \\ v_i & \text{otherwise} \end{cases}$$

where $v_i = p_i^{NL}$ if student i is not a legacy and $v_i = p_i^L$ if student i is a legacy. Combining these admissions rates with matriculation rates p_i^M (identical to those estimated above), the probability that each student attends in this counterfactual is $\tilde{v}_i * p_i^M$. We then select scaling parameters F_1 and F_2 such that the share of students in the attending class from the bottom 60% and from between the 60th and 95th income percentiles (each) match the shares obtained in the 4th row; these scaling factors are $F_1 = 2.3$ and $F_2 = 1.6$. Proportionally reducing admissions rates to maintain the same size of the class (overall scaling factor $F^* = \frac{N}{\sum_i \tilde{v}_i * p_i^M}$), we use an attendance weight for each student that is

$$v_i^{cf} = \tilde{v}_i * p_i^M * F^*$$

Scaling to Ivy-Plus Distribution. Finally, we take the parent income distributions we have calculated using the set of schools for which we have internal admissions data and scale these distributions to match the observed parent income distribution of all Ivy-Plus attendees according to the following process:

Step 1: We calculate the share of Ivy-Plus attendees with parent income in the four groups we report in Table 5a: bottom 60%, 60th-95th percentiles, 95th-99th percentiles, and top 1%. In particular, the share of Ivy-Plus attendees from the top 1% is 15.8%. We then calculate the counterfactual parent income distribution of Ivy-Plus attendees that would prevail if students with parent income above the 80th percentile attended Ivy-Plus institutions at the same rate as students with parent income between the 70th and 80th percentiles with the same test scores (unlike the decomposition analysis for Table 2 and Appendix Table 6, in which we change the attendance rates only for top 1% students). We call this the “equal attendance counterfactual”. In this counterfactual, the share of attendees from the top 1% is 7.2%. We calculate the same actual and counterfactual parent income distributions for the schools for which we have internal data, even-weighting across these schools.

We also calculate the intermediate parent income distributions at these schools after each of our counterfactual policy simulations. When removing advantages for legacy applicants and recruited athletes, we even-weight across the schools. When removing the influence of parent income in non-academic ratings and in calculating the need-affirmative counterfactual, we calculate the intermediate parent income distributions only for the school for which we have the most granular ratings data.

Step 2: We scale the intermediate policy counterfactual income distributions according to the overall difference between the actual and counterfactual distributions for all Ivy-Plus schools. We begin by calculating the difference between the share of students from the top 1% in the actual and attendance counterfactual classes, at all Ivy-Plus schools together (call this $\Delta top1_{full}^{ivy}$) and at each of our internal schools separately (call this $\Delta top1_{full}^s$). We calculate $\Delta top1_{full}^{ivy}$ as 8.6pp. For each of our internal schools, we also calculate the difference in top 1% shares between the legacy counterfactual and the full attendance counterfactual classes (call this $\Delta top1_l^s$). We then calculate the difference between $\Delta top1_{full}^s$ and $\Delta top1_l^s$ in log-points, even-weighting across schools. This difference is 28.3 log-points. We then apply these 28.3 log-points to the overall $\Delta top1_{full}^{ivy}$ (8.6pp) to calculate a difference of 6.5pp between the share of top 1% attendees in the full attendance counterfactual and the share of top 1% attendees in the legacy counterfactual. This implies that removing legacy preferences in admissions according to our simulation would reduce the share of Ivy-Plus attendees from the top 1% by 2.1pp, from 15.8% to 13.7%.

We then move to the second policy counterfactual: removing the influence of parent income on non-academic ratings. As in the legacy counterfactual, we calculate the difference top 1% shares between the ratings counterfactual and the full attendance counterfactual classes at the school for which we estimate the ratings counterfactual (call this $\Delta top1_r^s$). We calculate the difference between $\Delta top1_{full}^s$ and $\Delta top1_r^s$ as 68.4 log-points. Since we calculate this counterfactual using only one school, we translate this difference to the Ivy-Plus distributions using the difference in top 1% attendance attributable to admissions in the

decomposition (50.4 log-points), the difference attributable to legacy preferences in the decomposition (30.1 log-points, even-weighting across schools), and the difference attributable to non-academic ratings (27.5 log-points, at the school for which we calculate the ratings counterfactual). We calculate the remaining log-points attributable to admissions after removing legacy preferences ($50.4 - 30.1$), which we had been attributing to non-academic ratings in the decomposition. We scale this number by dividing by the contribution of non-academic ratings for this particular school (27.5 log-points) and multiplying this by the 68.4 log-points above to get a difference between $\Delta top1_{full}^{ivy}$ and $\Delta top1_r^{ivy}$ of 50.4 log-points. We apply these log-points to the $\Delta top1_l^{ivy}$ (6.5pp) to calculate a difference of 3.9pp between the share of top 1% attendees in the full attendance counterfactual and the share of top 1% attendees in the ratings counterfactual. This implies that removing the influence of parent income on non-academic ratings according to our simulation would reduce the share of Ivy-Plus attendees from the top 1% by 2.6pp, from 13.7% to 11.1%.

Moving to the third policy counterfactual in which we remove athletic preferences in admissions, we calculate the share of athletes from top 1% families for the schools for which we have internal data (18.3%, even-weighting across schools), and the share of the attending class that are athletes (14.6%). Because our earlier counterfactuals have de-admitted and refilled the class only among non-athletes, the share of athletes in the ratings counterfactual class is also 14.6%, and the share of non-athletes is 85.4%. We multiply the share of athletes in the class by the share of athletes from the top 1%, subtract this number from the overall share of the class from the top 1% in the ratings counterfactual (11.1%), and divide by the share of non-athletes in the class to calculate that 9.9% of the Ivy-Plus class is from the top 1% in the athletes counterfactual.

Step 3: We calculate the shares of the remaining parent income groups in the Ivy-Plus attendee distribution in our counterfactuals. For the legacy counterfactual, we first calculate the difference, in percentage points, between the share in income group p in the actual class and the share in income group p in the legacy counterfactual for the schools for which we have internal data, even-weighting across schools (call this $d_{p,l}^s$). We then calculate a scaling factor as the ratio of the difference in top 1% shares between the actual Ivy-Plus distribution and the legacy counterfactual Ivy-Plus distribution (15.8% - 13.7%) to the ratio of this difference for the schools for which we have internal data, again even-weighting across schools. For each income group p , we multiply $d_{p,l}^s$ by this scaling factor to calculate $d_{p,l}^{ivy}$, the difference in the shares of actual Ivy-Plus attendees from parent income bin p and the share of Ivy-Plus attendees from parent income bin p in the legacy counterfactual. We then subtract this difference from the share of actual Ivy-Plus attendees from parent income bin p to get the share of Ivy-Plus attendees from parent income bin p in the legacy counterfactual. We follow a similar process for the ratings and athletes counterfactuals, except we do not average across schools for which we have internal data in calculating $d_{p,r}^s$ and $d_{p,a}^s$. Instead, we use the college for which we calculate the ratings counterfactual.

Step 4: We then use the distribution of Ivy-Plus attendees in the athletes counterfactual to calculate the distribution of Ivy-Plus attendees in the need-affirmative counterfactual. As we had calibrated the need-

affirmative counterfactual such that the shares of the class from the bottom 95% matched the shares in the athletes counterfactual at the school for which we calculated the need-affirmative counterfactual, we similarly preserve the shares of Ivy-Plus attendees from the bottom 95% in the need-affirmative counterfactual. We then calculate the shares of students from the top 1% and from parent income percentiles 95-99. To do this, we first calculate two numbers for this select school: the share of students from the top 5% who have parent income between the 95th and 99th percentiles in the observed class and this share in the athletes counterfactual class. We calculate the difference between these shares and subtract it from the share of top 5% Ivy-Plus attendees in the athletes counterfactual who have parent income between the 95th and 99th percentiles. We apply this fraction to the share of Ivy-Plus attendees from the top 5% to calculate the share of Ivy-Plus attendees with parent income between the 95th and 99th percentiles in the need-affirmative counterfactual, so that the percentage point change in the share of the top 5% from percentiles 95-99 between the observed distribution and the athletes counterfactual distribution at the select school matches this percentage point change in the overall Ivy-Plus distributions. The remainder are from the top 1%.

Step 5: Because we are only able to calculate the ratings and need-affirmative counterfactuals using the college for which we have the most granular ratings data, Columns 5-7 of Table 5a report average outcomes for this college in Rows 1-5. We use the counterfactual attendance weights for this college to calculate each weighted average.

G High School Effects on Ratings and Admissions

In this appendix, we characterize the role of high schools in mediating differences in ratings and admissions probabilities by parental income.

We first estimate high school effects on admissions and examine their association with academic and non-academic ratings. To do so, we regress an indicator for Ivy-Plus admission on high school fixed effects, controlling a quintic in test scores, and indicators for race, gender, and parental income group, excluding the student herself to avoid mechanical biases. The resulting high school fixed effects can be interpreted as the difference in Ivy-Plus admissions rates across high schools for students with comparable test scores and demographics.⁴⁸

High school admissions fixed effects vary significantly across types of high schools. We divide high schools into four categories: non-religious private, religious private, and “advantaged” (typically affluent) vs. “disadvantaged” (typically lower-income) public high schools.⁴⁹ Advantaged public high schools – the types of schools most Ivy-Plus applicants from the middle class or upper middle class attend – have the lowest fixed

⁴⁸To obtain estimates with adequate precision, we restrict attention to schools that have at least 40 non-legacy, non-athlete Ivy-Plus applicants in our sample. Note that these high school effects cannot be interpreted as the causal effects of high school attendance on college admissions because we do not attempt to fully control for selection of students across schools.

⁴⁹We break public high schools into two groups based on their percentile on high school challenge indicators that capture educational opportunities or disadvantages in the high school environment, variables that feed into the CollegeBoard Landscape tool (Mabel et al. 2022; Bastedo et al. 2022). We classify high schools that fall in the top 20% of this index of advantage as “advantaged.” 75% of applicants to our Ivy-Plus colleges come from advantaged high schools with this definition.

effects; disadvantaged public high schools and religious schools are in the middle; and private high schools have the most positive fixed effects. The differences are substantial in magnitude: students attending non-religious private high schools are twice as likely to be admitted to an Ivy-Plus college as those who attend advantaged public schools with comparable test scores and demographics (Appendix Figure 17a).⁵⁰ Since students from the top 1% are more likely to attend private high schools, they attend schools that have much more positive admissions fixed effects than middle-class students (Appendix Figure 18a).

Tying these high-school-level differences back to admissions office ratings, we find that the higher admissions rates at the schools attended by children from high-income families arise entirely from differences in *non-academic* rather than academic factors. Appendix Figure 18b plots the share of students with high non-academic and academic ratings by ventiles of estimated high school fixed effect, reweighting on test score.⁵¹ About 61% of children receive high academic ratings, irrespective of whether they attend a high school with a small or large admissions fixed effect. In contrast, the share of students receiving high non-academic ratings rises from 15% to nearly 40% going from the schools with the lowest to highest admissions fixed effects, partly because schools with higher admissions fixed effects generate more positive teacher recommendation and guidance counselor letters (Appendix Figures 18c and 18d). Consistent with the results in Appendix Figure 17a, students at private high schools have much higher non-academic ratings (but no higher academic ratings) than peers with comparable test scores and demographics at other schools (Appendix Figures 17b and 17c). In short, the admissions advantage for students from the private high schools typically attended by higher-income families appears to arise not from having a stronger academic program (e.g., more advanced classes), but rather from non-academic distinctions outside the classroom.

H Multiple-Rater Test for Idiosyncratic Variation in Admissions

This appendix presents a proof of the following claim made in Section 4.1: if admissions decisions at college B are orthogonal to those at college A conditional on X_{1i} and \tilde{X}_{2i} , then equation (5) yields unbiased estimates of the causal effect of admission to A :

$$T_{B|A} = 0 \implies r_A = \phi_A.$$

Proof. We will establish that if $Var(\epsilon_{2i}^X) > 0$, then the probability of admission to college B is correlated with whether a student is admitted to college A under Assumption 1. To simplify notation, let $\tilde{C}_j = C_j - \gamma_{1j}X_{1i} - \gamma_{2j}\tilde{X}_{2i}$ denote the threshold for admission adjusting for observable characteristics at college

⁵⁰Although the two-fold difference in admissions rates between students who attend non-religious private high schools vs. public schools is smaller than the four-fold boost in admissions from being a legacy applicant at any given Ivy-Plus college, the effect of attending a private high school on an applicant's overall chances of attending an Ivy-Plus college may be larger than the legacy boost because applicants who attend private high schools have higher admissions rates at *all* Ivy-Plus colleges, whereas the legacy advantage applies only at the colleges that the applicant's parents attended.

⁵¹To adjust for noise in the fixed effect estimates, we shrink the estimates by multiplying each fixed effect by the ratio of signal variance to the high-school-specific total variance. We estimate the signal variance by splitting the sample of applicants into even and odd application years, estimating high school fixed effects separately for each sample, and then calculating the signal variance as the covariance between these separately estimated fixed effects. We estimate the total variance of each high school's fixed effect as the sum of signal variance and that school's noise variance, the squared standard error of its fixed effect when estimated on the full sample of students.

j , and $\tilde{X}_i = (X_{1i}, \tilde{X}_{2i})$ denote the vector of observable characteristics.

The probability of admission to college B conditional on admission to college A for a student with characteristics \tilde{X}_i is:

$$\begin{aligned} E[P_{iB} = 1 | P_{iA} = d, X_{1i}, \tilde{X}_{2i}] &= E[Z_{iB} > C_B | P_{iA} = 1, X_{1i}, \tilde{X}_{2i}] \\ &= E[\gamma_{2B}\epsilon_{2i}^X + \eta_i + \epsilon_{iB} > \tilde{C}_B | P_{iA} = 1, \tilde{X}_i] \\ &= E[\gamma_{2B}\epsilon_{2i}^X + \eta_i + \epsilon_{iB} > \tilde{C}_B | \gamma_{2A}\epsilon_{2i}^X + \eta_i + \epsilon_{iA} > \tilde{C}_A, \tilde{X}_i] \end{aligned}$$

The difference in the probability of admission to college B conditional on being accepted vs. rejected at college A is therefore:

$$T_{B|A} = E[\gamma_{2B}\epsilon_{2i}^X + \eta_i + \epsilon_{iB} > \tilde{C}_B | \gamma_{2A}\epsilon_{2i}^X + \eta_i + \epsilon_{iA} > \tilde{C}_A, \tilde{X}_i] - E[\gamma_{2B}\epsilon_{2i}^X + \eta_i + \epsilon_{iB} > \tilde{C}_B | \gamma_{2A}\epsilon_{2i}^X + \eta_i + \epsilon_{iA} < \tilde{C}_A, \tilde{X}_i].$$

Since $\epsilon_{iA} \perp \epsilon_{iB}$ and $\epsilon_{2i}^X \perp \epsilon_{ij}$, it follows that if $Var(\epsilon_{2i}^X) > 0$ and $\gamma_{2A} > 0$, then $\gamma_{2B} > 0$ implies that $T_{B|A} > 0$, i.e., the probability of admission to B differs depending upon whether a student is admitted to A .

I Validation of Early-Career Proxies for Leadership Outcomes

This appendix presents further details on how we estimate the likelihood ratios

$$R_{PS} = \frac{P(Y_i^P | Y_i^S = 1, \text{Ivy-Plus or Flagship Public})}{P(Y_i^P | Y_i^S = 0, \text{Ivy-Plus or Flagship Public})}$$

reported in Table 3, which we use to validate the early-career proxies for various leadership outcomes in 4.2.

In Panel A of Table 3, we measure the likelihood ratio for business leadership outcomes. We obtain information on Fortune 500 CEOs and corporate board and committee members from BoardEx, a dataset covering executives and directors from roughly 17,400 firms, restricting to those employed between 2015 and 2024. These data, combined with data from Databahn (2024) on Fortune 500 CEOs, provide information on college attendance and graduate school attendance, which we use to estimate the likelihood ratio for the elite graduate school proxy for these outcomes.

In Panel B of Table 3, we use data on the educational backgrounds of all Nobel laureates from 1980-2015 from Wai et al. (2024) to measure the likelihood ratio for all U.S.-born Nobel prize winners for the elite graduate school proxy. We obtain information on Nobel laureates' birthplaces from Wikipedia.

In Panel C, we use publicly available data from Wikipedia and other sources on the careers of Supreme Court justices and secretaries of the U.S. Treasury to obtain information on their educational backgrounds and employment history. Because the exact list of prestigious firms constructed internally using tax records cannot be disclosed externally, we define prestigious firms for this application by combining the 2025 Vault Law 100 Firms, 2024 Vault Most Prestigious Banking Firms, 2024 US News Top 10 Law Schools, and 2024 Top 10 RePEc US Economics Departments. As discussed in Section 2.3, there is substantial overlap between these public lists and our internal definition of prestigious firms.

J Heterogeneity of Effect of Ivy-Plus Admissions by Outside Options

This appendix describes how we construct Figure 10 and Appendix Table 9, which show how the treatment effect of Ivy-Plus admission from the waitlist varies with the strength of an applicant’s outside options.

We begin by estimating colleges’ observational value-added using OLS regressions of a student’s predicted top 1% probability on fixed effects for the college students attend, controlling for parental income, test scores, race, birth cohort, and home state. We estimate these regressions using our pipeline analysis sample and normalize VA for the average highly selective state flagship public college (listed in Appendix Table 1) to 0.

We then place Ivy-Plus applicants into subgroups j based on their home state, parent income, race, and the Ivy-Plus college to which they applied. Within each group j , we calculate each student’s gain g_j from attending the relevant Ivy-Plus college as the observational value-added (VA) of the Ivy-Plus college to which she applied minus the mean observational value-added of colleges attended by those rejected from the waitlist in that group. We then define the implied mean observational value-added of a student’s outside options as the mean observational VA of the Ivy-Plus colleges in our college-specific sample minus the student-specific gain from Ivy-Plus attendance g_j ; intuitively, this variable measures the strength of a student’s outside options relative to the value-added of the average Ivy-Plus college for which we have data.

To construct Figure 10, we divide students into 20 bins based on the implied strength of the outside options variable. The x coordinate of each of the 20 points is the mean implied observational VA of outside options within each bin. To construct the y coordinates, we regress the predicted top 1% outcome on indicators for Ivy-Plus admission interacted with the 20 outside option strength dummies and indicators for the Ivy-Plus college to which they applied, using the sample of waitlisted Ivy-Plus applicants as in Figure 8b. In order to obtain a visual representation that is aligned with the 2SLS regression coefficient that we report in Appendix Table 9 (“visual IV”), we then divide these coefficients by the “first stage” effect of the strength of outside options on actual college VA, i.e., the coefficient on the interaction term in a regression of observational VA of the college a student actually attends on an indicator for Ivy-Plus admissions, the gain from attending an Ivy-Plus college relative to outside options, the interaction of those two variables, and indicators for the Ivy-Plus college to which the student applied.

We also report the 2SLS regression slope (and the implied best fit line) corresponding to the plotted points, estimated using a 2SLS regression of the predicted top 1% outcome on the observational VA of the college a student actually attends (multiplying the coefficient by -1 since the x variable is the implied outside option rather than the gain in the figure). We instrument for observational VA with the interaction between the gain in observational VA from Ivy-Plus admission g_j and an admissions indicator, controlling for the admissions indicator, g_j , and indicators for the Ivy-Plus college to which the student applied among waitlisted students. Column 1 of Appendix Table 9 reproduces this specification; the subsequent columns of the table report variants with different approaches to constructing the instrument g_j , as detailed in the notes to that table.

K Methodology for Estimating Differences in Potential Outcomes by Application Credentials

This appendix formalizes the methodology used in Section 5 to estimate differences in potential outcomes by application credentials.

Let Y_i^{Ivy} denote student i 's post-college outcome (e.g., earnings) if she attends an average Ivy-Plus college. Our goal is to identify the average difference in Y_i^{Ivy} for applicants with different credentials X_{1i} , in order to understand how changing who is admitted would affect the average level of post-college outcomes for Ivy-Plus students. For example, we are interested in identifying the difference in outcomes for legacies ($X_{1i} = 1$) vs. non-legacies ($X_{1i} = 0$) among Ivy-Plus applicants:

$$\Delta Y_X = E[Y_i^{Ivy}|X_{1i} = 1] - E[Y_i^{Ivy}|X_{1i} = 0] \quad (7)$$

We cannot estimate (7) directly because we do not observe Y_i^{Ivy} for students who do not attend Ivy-Plus colleges. Instead, we infer students' potential outcomes had they attended Ivy-Plus colleges by combining our estimates of colleges' causal effects with students' observed outcomes. Under the model in (4), we can write a student's post-college outcome (e.g., earnings) as

$$Y_i = \phi_{j_D(i)} + \omega_i, \quad (8)$$

where $\phi_{j_D(i)}$ denotes the value-added of the college that student i attends and $\omega_i = \beta_1 X_{1i} + \beta_2 X_{2i} + \epsilon_i^Y$ denotes the student's latent potential, defined here as the student's outcome if she were to attend the average flagship public college (for which $\phi_j = 0$). Importantly, (8) assumes that there is no heterogeneity in college value-added across students, an assumption consistent with the tests for heterogeneity in treatment effects implemented in Section 4.

We can write each student's potential outcome had they attended an Ivy-Plus college as

$$Y_i^{Ivy} = \omega_i + \phi_{Ivy} = Y_i + \phi_{Ivy} - \phi_{j_D(i)} = Y_i + \Delta\phi_{j_D(i)}, \quad (9)$$

where ϕ_{Ivy} denotes the mean value-added of the Ivy-Plus colleges in our college-specific sample and $\Delta\phi_{j_D(i)}$ is the difference between ϕ_{Ivy} and the value-added of college that student i attends. Intuitively, we infer students' potential outcomes at Ivy-Plus colleges by adding the difference in the value-added of the average Ivy-Plus college and the college they actually attended to their observed outcome Y_i .

Since we have design-based estimates only for a subset of colleges, we estimate each college's value-added $\phi_{j_D(i)}$ using the observational model used in Section 4, regressing the relevant outcome (e.g., an indicator for reaching the top 1%) on college fixed effects and a quintic in test scores, 13 parent income bins, and indicators for race, gender, and home state. As discussed in Section 4, these observational estimates $\phi_{j_D(i)}^{obs}$ are highly correlated with the causal estimates obtained from our two research designs. To adjust for the remaining selection in the observational estimates, we multiply the raw fixed effects by ψ_Y , the ratio of the

causal effect estimates from our waitlist design to the corresponding observational estimate for outcome Y (e.g., a coefficient of $\psi_Y = 0.87$ for the predicted top 1% outcome, as shown in Figure 10a).

Using these estimates of $\Delta\hat{\phi}_{j_D(i)} = \psi_Y (\phi_{Ivy}^{obs} - \phi_{j_D(i)}^{obs})$ to estimate $\hat{Y}_i^{Ivy} = Y_i + \Delta\hat{\phi}_{j_D(i)}$, we obtain the following feasible estimator for the difference in potential outcomes between applicants with different credentials:

$$\Delta Y_X = E[\hat{Y}_i^{Ivy}|X_{1i} = 1] - E[\hat{Y}_i^{Ivy}|X_{1i} = 0]. \quad (10)$$

This estimator is unbiased assuming that each college's value-added is homogeneous across students and that the relationship between observational VA measures and causal effects within the subsample of colleges where we have design-based estimates ψ_Y is constant across all colleges.⁵² To evaluate the sensitivity of our conclusions to these assumptions, we compare the outcomes of Ivy-Plus *attendees* (rather than applicants) with different application credentials, an approach that does not require any use of college value-added estimates, but instead relies on other assumptions to rule out selection bias among admitted students (see Appendix L). This alternative approach yields very similar results (Appendix Figure 35), indicating that the results that follow do not rest on the specific way in which we measure and adjust for college value-added.

L Outcome Comparisons Among Ivy-Plus Attendees

In this appendix, we replicate the outcome-based tests in Section 5 by comparing outcomes among Ivy-Plus attendees. We show that this approach – which does not rely on assumptions about colleges' causal effects – yields results very similar to our baseline estimates that correct for selection using college value-added estimates.

Methods and Assumptions. We estimate ΔY_X by comparing the observed outcomes of Ivy-Plus attendees with different credentials (e.g., legacy vs. non-legacy students):

$$\Delta Y_{X|Ivy} = E[Y_i|X_{1i} = 1, j_i = Ivy] - E[Y_i|X_{1i} = 0, j_i = Ivy] \quad (11)$$

By conditioning on attending an Ivy-Plus college, this comparison holds fixed college value-added, thereby isolating differences in students' potential ω_i independent of college fixed effects. However, because the set of students who are admitted to Ivy-Plus colleges is endogenously selected based on their overall rating as in (3), this estimator does not necessarily yield an unbiased estimate of the average difference in outcomes among legacy and non-legacy students in the applicant pool had they all attended Ivy-Plus colleges (ΔY_X) (see Rothstein 2004 for a more general discussion of this issue). Intuitively, given legacy preferences, non-legacy students who are admitted must have a more positive draw on some other attribute on average (e.g., academic credentials) in order to gain admission to an Ivy-Plus college. If those attributes are correlated with

⁵²This approach can be interpreted as a mediation analysis that seeks to isolate how much of the difference in outcomes by a credential such as legacy status is driven by college attendance (the mediator). We use estimates of the causal effects of college on long-term outcomes to subtract out the contribution of colleges to the differences in long-term outcomes between legacy vs. non-legacy Ivy-Plus applicants rather than simply conditioning on the endogenous mediator (college attended).

long-term outcomes (X_{2i}), we will obtain an estimate $\Delta Y_{X|Ivy} < \Delta Y_X$, since we are effectively comparing non-legacy students with stronger academic credentials on average to legacy students within the admitted pool of students. To obtain an unbiased estimate of ΔY_X by comparing outcomes among matriculants, we must therefore make the following strong assumption, which rules out the preceding example and assumes that all residual variation in admissions decisions comes from idiosyncratic factors unrelated to long-term outcomes.

Assumption 3 (Idiosyncratic Admissions Conditional on Observables). *Conditional on X_1 , differences in admissions decisions are driven entirely by idiosyncratic factors ϵ_{ij} rather than latent unobservables correlated with long-term outcomes X_{2i} : $Var(X_{2i}) = 0$.*

Although this assumption may not hold exactly, the estimator in (11) turns out to yield estimates that are very similar to our baseline estimates that adjust for selection bias among all applicants.

Results. We regress outcomes on indicators for legacy status, being a recruited athlete, having high non-academic ratings, and having high academic ratings in our college-specific sample of Ivy-Plus attendees. Appendix Figure 35 plots the coefficients obtained from this OLS regression along with 95% confidence intervals for the same three outcomes that we consider in Figure 14. We find very similar results, with nearly identical magnitudes. Legacy status and non-academic ratings have no significant association with any of the three outcomes. Athletic recruitment is unrelated to income but negatively associated with the probability of attending an elite graduate school or working at a prestigious firm. By contrast, having a high (above-median) academic rating is strongly associated with significantly better outcomes, with magnitudes similar to the causal effect of attending an Ivy-Plus college instead of a state flagship college. These findings show that our baseline results hold even when we make simple comparisons among Ivy-Plus attendees – perhaps because a large portion of the variation in admissions decisions is driven by idiosyncratic factors conditional on the observable factors we consider – and thus do not depend on the way in which we adjust for college value-added or on assumptions about the heterogeneity of colleges’ causal effects across students.

M Predicting Impacts on Backgrounds of Society’s Leaders

This appendix presents further details underlying the methods used in Section 6.2 to calculate the effect of admissions policy changes on the parental income distribution of individuals who achieve various post-college outcomes.

Changes in Early-Career Outcomes. We first calculate how admissions policy reforms at Ivy-Plus colleges would affect the socioeconomic backgrounds of students who achieve early-career outcomes that we measure directly in the data. Let β_o denote the causal effect of Ivy-Plus attendance on outcome o . We assume this causal effect does not vary with parental income.

If an admissions reform increases the fraction of students from the bottom 60% of the parent income distribution at Ivy-Plus schools by $\Delta_{Bottom60}$, then there will be $\Delta_{Bottom60}\beta_o$ more such students who achieve

outcome o . We then apply Bayes Rule to arrive at the change in the socioeconomic backgrounds of students achieving outcome o :

$$\Delta P(\text{Bottom 60\%} | \text{Outcome } o) = \Delta_{\text{Bottom60}} \times \beta_o \times \frac{P(\text{Ivy-Plus})}{P(\text{Outcome } o)}$$

Rows 1 and 4 of Table 5b present calculations using this approach to calculate the impact of different admissions scenarios on the share of students reaching the top 1% of the income distribution at age 33 and attending an elite graduate school, respectively.

Counterfactuals Including Other Elite Private Colleges. We predict the effects of broader admissions policy reforms across all elite private schools (defined as the 60 other private schools listed in Tier 2 in the Mobility Report Cards data) by making use of publicly available data from Chetty et al. (2020). As with the Ivy-Plus colleges, we assume that flagship public colleges (as the elastically supplied college places) are the relevant outside option. We lack direct estimates of the causal effect of attending elite private vs. public colleges; instead, we assume that the causal effect from attending elite private colleges is proportionally smaller than that from attending Ivy-Plus colleges based on the lower observed rate at which students from elite private schools reach the top 1%. In practice, because 9.5% of students from elite private schools reach the top 1% of the income distribution at age 33, compared with 19.4% for students from Ivy-Plus colleges, we extrapolate a causal effect of $\beta_{\text{Top1}}^{\text{ElitePriv}}$ (i.e., 49% of the Ivy-Plus effect). We then adapt the formula above to calculate the change in the socioeconomic backgrounds of top 1% earners resulting from changes in admissions policies at all elite private institutions:

$$\Delta P(\text{Bottom 60\%} | \text{Top1\%}) = \Delta_{\text{Bottom60}} \times \beta_{\text{Top1}}^{\text{ElitePriv}} \times \frac{P(\text{Elite Private})}{P(\text{Top1\%)}}$$

Changes in Other Leadership Outcomes. Finally, we examine how admissions policy reforms would affect the socioeconomic backgrounds of individuals achieving upper tail success on other, non-monetary dimensions (e.g., becoming a Supreme court justice). We extrapolate from our treatment effect estimates on early-career outcomes to such longer-term leadership outcomes by extrapolating from our treatment effect estimates on the chances of reaching the top 1% (for monetary/business outcomes), attending an elite graduate school (for Arts and Sciences outcomes), or working at a prestigious firm early in one's career (for Public Service outcomes). For example, our estimates in Figure 13 imply that 85% of the observational difference in rates of working at prestigious firms between students who attend Ivy-Plus vs. state flagship colleges is due to a causal treatment effect of Ivy-Plus attendance. We assume that this 85% ratio is the same for long-term public service leadership outcomes, i.e., 85% of the observational difference between the rates at which students from Ivy-Plus and flagship public colleges attain leadership outcomes is due to causal effects.

Using Bayes rule, we estimate the base rates of achieving a leadership outcome conditional on Ivy-Plus attendance or flagship public attendance using the statistics that we present in Figure 1 and A.1 on the fraction of leaders from particular college backgrounds. For example, for Ivy-Plus students, the observed rate is

$$P(Outcome|Ivy-Plus) = P(Ivy-Plus|Outcome) * \frac{P(Outcome)}{P(Ivy-Plus)}.$$

We can then estimate the implied causal effect as a function of these statistics:

$$\beta_o = 85\% \times (P(Ivy-Plus|Outcome) * \frac{P(Outcome)}{P(Ivy-Plus)} - P(Flagship Public|Outcome) * \frac{P(Outcome)}{P(Flagship Public)})$$

Plugging this causal effect into our formula from above yields

$$\Delta P(Bottom\ 60\%|Outcome) = \Delta_{Bottom60} \times 85\% \times (P(Ivy-Plus|Outcome) - P(Flagship Public|Outcome) * \frac{P(Ivy-Plus)}{P(Flagship Public)})$$

Note that the term in parentheses is simply the difference in representation among leaders of individuals from Ivy-Plus vs. flagship public schools, adjusting the representation of the latter for the difference in the size of the institutions.

All other rows in Table 5b (other than the first row) present calculations using this approach for different admissions scenarios for key leadership outcomes from Figure 1.

Note that the approach we take differs from commonly used surrogate index methods (Athey et al., 2019), which would predict impacts on leadership outcomes by multiplying the causal effect on the early-career proxy by a second-stage coefficient from a regression of the leadership outcome on the early-career proxy. This approach would yield unbiased estimates of impacts of leadership outcomes under the surrogacy assumption that the early-career proxy fully captures the mediating pathway between Ivy-Plus attendance and the leadership outcome. We believe this condition is unlikely to hold in practice in this application, as Ivy-Plus attendance likely impacts leadership outcomes through many pathways that do not run through our three early-career markers of success. We therefore instead assume that the causal share of the observational difference in outcomes between students who attend Ivy-Plus vs. flagship public colleges is the same across outcomes (through whatever pathway) and then rescale the observational difference in the leadership outcomes of interest by the causal share estimated for the early career proxies.

N Impacts of Ivy-Plus Attendance on Tax Revenue

This appendix describes our method for calculating the effects of Ivy-Plus attendance on total earnings and tax revenues. We estimate two partial-equilibrium parameters: first, the net present value (NPV, discounted to age 19) of additional earnings and federal income taxes paid by a single cohort of Ivy-Plus students over their careers, relative to a counterfactual in which they had attended flagship public colleges; and second, the total additional income earned and federal income taxes paid in 2025 by past Ivy-Plus students, again relative to a counterfactual in which all had attended flagship public colleges. We ignore any general

equilibrium effects of a change in the number of Ivy-Plus students, either within a given cohort or overall in the workforce.

To estimate these two parameters based on our treatment effects on mean incomes at age 33, we follow the prior literature (e.g., Chetty et al., 2011 and Chetty, Friedman, and Rockoff, 2014) and make the following assumptions: (1) a constant (proportional) causal effect on incomes across the life-cycle (41.3%), and (2) an Ivy-Plus life-cycle earnings trajectory that (proportionally) follows the cross-sectional age-earnings profile for US individuals. Together, these assumptions allow us to scale our estimated effect of Ivy-Plus attendance on mean income at age 33 (\$101,000, see Figure 28a) to a full life-cycle profile of causal effects (beginning at age 26; we assume no causal effects prior to age 26).

Next, we assume an average marginal tax rate on these additional earnings of 30%. As shown in Appendix Figure 28a, most earnings gains accrue to earners already in the top income tax bracket (37% rate for incomes above \$751,600 for married couples), but a significant share of these additional earnings accrue as capital gains (taxed at 23.8% for top income earners). We therefore use 30% as an approximate average marginal tax rate. Multiplying the 30% rate by the additional incomes (as calculated above) produces a life-cycle distribution of additional tax revenue.

The net present value of the life-cycle flow of additional tax payments (discounted at a rate of 3% net of growth back to age 19) is \$615,000 per student. There were 19,689 total students in the 2013 cohort at Ivy-Plus colleges. The \$615,000 gain in tax revenues per student implies total NPV federal tax gains of \$12.12 billion per cohort.

Finally, we calculate the total value of additional federal taxes paid by Ivy-Plus students in 2025. Past Ivy-Plus cohorts were somewhat smaller; accounting for growth of roughly 0.5% over the past fifty years (which is equivalent to discounting cash flows at 0.5% per year instead of 3% per year) yields a total earnings impact of \$80.32 billion per year across all Ivy-Plus graduates and total additional tax revenue of \$24.10 billion in 2025 (measured in real 2015 dollars).

O Pipeline Statistics by College

In this appendix, we describe the methods we use to calculate attendance and application rates at the college-specific level, which we release publicly along with this study. We calculate and report attendance and application rates for all Ivy-Plus institutions, the schools used in our flagship public and elite private school samples, the members of the New England Small College Athletic Conference, flagship public universities, and nearly all of the remaining colleges ranked in the top 100 national universities by U.S. News and World Report in 2022.

We begin with the merged dataset, as described in Section 2, for students who took a standardized test and were on track to graduate from high school in the classes of 2011, 2013, or 2015. We calculate attendance rates (fraction of students who attend each college) separately for students in each college-by-parent income bin-by-test score cell. We use 13 parent income bins, corresponding to parent income rank percentiles 0-20,

20-40, 40-60, 60-70, 70-80, 80-90, 90-95, 95-96, 96-97, 97-98, 98-99, 99-99.9, and the top 0.1%. We convert ACT scores into SAT scores using concordance tables published by the College Board and the ACT. We then aggregate these attendance rates to the college-by-parent income bin level using the distribution of test scores for students attending a given institution. More specifically, in calculating the attendance rates for a given college c , we weight test-takers with score a from parent income group p with

$$weight_{acp} = \frac{P(SAT_a|attend_c)}{P(SAT_a|p)} \quad (12)$$

This ensures that the distribution of test scores matches that of attending students at a given college for all parent income groups. For public universities, we also calculate attendance rates separately for in-state and out-of-state students using a very similar process but with two differences. First, in each case, the test-score distribution used for the final weighting remains the distribution of test scores for all attending students rather than of in-state or out-of-state students; second, in order to avoid statistics based on very few students in our school-specific analyses, we calculate a single statistic for attendance and application for in-state students from the top 1% (thereby combining those from the 99-99.9 and top 0.1% parent income bins).

We cannot directly observe applications for schools for which we do not have internal admissions data. However, our data from testing companies includes a subset of schools to which students submitted their scores. We observe up to 33 score submissions for each student who took the SAT or the ACT. Sending test scores to a college was generally required as part of the application process during the years for which we calculate attendance and application rates; however, a record of score submission (“scoresend”) to a school does not guarantee that the student applied to that school, nor does the absence of scoresend rule out an application.

We therefore predict application rates at the college-by-parent-income-bin by score level using scoresend rates, supplemented with other data sources. We begin by predicting actual applications conditional on observed scoresends within the subset of schools for which we can observe completed applications. We then regress this indicator for completed application at the student level on quintics of test score, parent income rank, and distance from the college, defined as the distance from the college’s address to the centroid of the student’s home zip code.

We further validate our estimates using data from the Integrated Postsecondary Education Data System (IPEDS), which includes the number of applicants per year to many colleges that have published this information, for the years 2002-2020. We first adjust the total number of applicants in the IPEDS data to account for unlinked students (primarily international students) using the ratio of total number of applications in the internal data to the total number of applications in the IPEDS data for each year. We then take the average of these scaling factors, weighted by the number of applicants to each school, for each year, separately for public and private schools. We then apply these scaling factors to the IPEDS data for all schools. Then at the college by year level, we calculate an “application gap” as the difference between the scaled number

of applicants in the IPEDS data and the total number of predicted applicants from the scoresend model. We then scale each student's predicted probability of having completed an application by the ratio of this application gap to the total number of scoresends, distributing the application gap proportionally across all students who send scores. We then collapse these predicted application rates by parent income bin, using the same SAT weighting process as in the calculation of the attendance rates. As with attendance rates, we also calculate the predicted application rates separately for in-state and out-of-state students for public schools.

To protect the confidentiality of individuals in the tax data, we add a small amount of random noise to each statistic following a differentially private algorithm (Dwork et al., 2006 and Chetty and Friedman, 2019). Because each published statistic is a fraction, the global sensitivity of each number is $1/N$, where N is the number of individuals in our data in a given parent income bin. (For in-state estimates for public colleges, N is the number of individuals in our data in a given state and parent income bin; for out-of-state estimates, N is the number of individuals in our data excluding those from a specific state.) Using $\epsilon=1$, we then add random noise drawn from a normal distribution with mean 0 and standard deviation $1/N$.

Table 1: Summary Statistics for Analysis Samples

| | Sample | | | |
|---|-----------------|---------------------------|-------------------------|--|
| | Pipeline (1) | Long Term Outcomes (2) | Ivy-Plus (3) | Flagship Public College-Specific (4) |
| | | | College-Specific (3) | Flagship Public College-Specific (4) |
| <i>Panel A: College Attendance</i> | | | | |
| % Attending Any College | 93.0% | 96.3% | 98.0% | 99.0% |
| % Attending Ivy+ College | 0.7% | 0.9% | 24.3% | 4.6% |
| % Attending Flagship Public College | 2.4% | 2.6% | 11.3% | 24.9% |
| % Attending Other Selective Private College | 0.9% | 1.0% | 11.7% | 5.6% |
| <i>Panel B: Standardized Test Scores</i> | | | | |
| Mean Test Score | 991 | 993 | 1374 | 1228 |
| Mean Number of Scoresends to Colleges | 4.49 | 4.86 | 9.88 | 7.46 |
| <i>Panel C: Admission and Matriculation</i> | | | | |
| % Admitted | - | - | 13.0% | 34.4% |
| % Matriculated | - | - | 7.4% | 15.7% |
| % Applied Early | - | - | 12.4% | 29.1% |
| % Waitlisted | - | - | 10.4% | 4.3% |
| % Athlete | - | - | 1.7% | 0.3% |
| % Legacy | - | - | 3.5% | - |
| % Children of Faculty | - | - | 0.1% | - |
| <i>Panel D: Demographics</i> | | | | |
| Mean Year of Birth | 1994 | 1985 | 1990 | 1989 |
| Mean Age at Matriculation | 18 | 18 | 18 | 18 |
| % Female | 53.4% | 54.1% | 55.2% | 46.5% |
| % White | 57.4% | 66.1% | 50.9% | 45.8% |
| % Black | 13.1% | 10.6% | 7.6% | 6.3% |
| % Hispanic | 14.3% | 7.4% | 8.9% | 14.2% |
| % Asian | 5.8% | 5.1% | 20.9% | 26.3% |
| % American Indian/ Native American | 0.7% | 0.8% | 1.3% | 0.6% |
| % Native Hawaiian/ Pacific Islander | 0.1% | 0.0% | 0.1% | 0.0% |
| % Unknown Race | 8.6% | 10.0% | 10.4% | 6.7% |
| <i>Panel E: Parents' Incomes</i> | | | | |
| Mean Parent Household Income | \$76,360 | \$86,030 | \$151,627 | \$123,027 |
| Mean Parent Income Rank | 61.8 | 62.5 | 78.0 | 72.3 |
| <i>Panel F: Post-College Outcomes</i> | | | | |
| Median Income at Age 33 | - | \$43,238 | \$79,731 | \$68,753 |
| Mean Income Rank at Age 33 | - | 65.8 | 79.8 | 77.0 |
| % in Top 1% at Age 33 | - | 2.0% | 13.2% | 7.7% |
| Predicted Top 1% at 33 based on Age 22-25 Employers | 3.8% | 3.6% | 11.1% | 7.7% |
| Predicted Income Rank at Age 33 | 71.2 | 71.2 | 77.6 | 75.7 |
| % Attending Graduate School at Age 28 | 7.3% | 8.3% | 23.2% | 16.1% |
| % Attending an Elite Graduate School at Age 28 | 0.4% | 0.5% | 7.1% | 2.9% |
| % Working at an Elite Firm at Age 25 | 3.3% | 3.3% | 18.3% | 5.3% |
| % Working at a Prestigious Firm at Age 25 | 4.2% | 4.2% | 19.7% | 5.8% |
| Number of Children | 5,063,263 | 9,849,734 | 486,150 | 1,877,770 |

Notes: The table presents summary statistics for the samples defined in Section 2. Column 1 includes children who (1) have a valid Social Security Number (SSN) or Individual Taxpayer Identification Number (ITIN), (2) can be linked to parents, and (3) appear in either the SAT or ACT data in 2011, 2013, or 2015. Column 2 includes children who (1) have a valid Social Security Number (SSN) or Individual Taxpayer Identification Number (ITIN), (2) can be linked to parents, (3) were born in 1982-1988, and (4) appear in either the SAT or ACT data in 2001 to 2005 or 2007. Columns 3 and 4 show statistics for applicants to selected Ivy-Plus colleges (column 3) and flagship public colleges (column 4) from which we have internal application records and who (1) are US citizens or permanent residents, (2) can be linked to the tax data based on their SSN or ITIN, (3) can be linked to parents in the tax data, (4) were born before 1996, and (5) were 21 or younger when they applied to college. Test scores are reported in SAT points out of 1600 (with ACT scores converted to SAT points). For post-college outcomes in panel F, we further restrict to students old enough to achieve relevant outcomes. For example, row 1 of Panel F restricts to children born between before 1988. In columns 3 and 4, Panel C reports statistics counting each college application once; all other panels count each student once when constructing statistics, even if they apply to multiple schools. All monetary values are measured in 2015 dollars. See Section 2 for variable definitions and data sources.

Table 2: Sources of Additional Students from Top 1% at Ivy-Plus Colleges: Decomposition Analysis

| | | Total Students | Total Excess | Subtotal | Share of Excess Top 1% Students Cond. On Scores |
|------|---|----------------|--------------|----------|--|
| [1] | Class Size | 1650 | | | |
| [2] | Total Students with Parent Income in Top 1% | 261 | | | |
| [3] | Top 1% Students with Equal Attendance Rates Unconditionally | 16 | | | |
| [4] | Excess Students from Top 1% Unconditionally | | 245 | | |
| [5] | Top 1% Students with Equal Attendance Rates Cond. on Scores | 93 | | | |
| [6] | Excess Students from Top 1% Conditional on Test Scores | | 168 | | 100.0% |
| [7] | Attributable to Differences in Admissions | | 114 | | 68.1% |
| [8] | Legacy Preferences | | 52 | | 31.0% |
| [9] | Non-Academic Credentials | | 35 | | 20.9% |
| [10] | Recruited Athletes | | 27 | | 16.2% |
| [11] | Attributable to Differences in Matriculation | | 20 | | 11.9% |
| [12] | Attributable to Differences in Application | | 34 | | 20.1% |

Notes: This table quantifies the sources of the gap in attendance rates between students from families in the top 1% of the income distribution and students from families between the 70th and 80th percentile of the national income distribution (the "middle class") by considering a series of counterfactuals. Rows 5-12 hold test scores fixed, while Rows 3 and 4 do not. Row 1 reports the average class size for incoming first-year students at the 12 Ivy-Plus colleges in our sample period. Row 2 reports the observed number of students with parents in the top 1%. Row 3 shows the counterfactual number of students from the top 1% who would attend the average Ivy-Plus college if students from all parental income groups had an equal chance of attending. Row 4 presents the excess students from the top 1% relative to this benchmark, i.e. the difference between rows 2 and 3. Row 5 calculates the counterfactual number of students from the top 1% who would attend the average Ivy-Plus college if students from the top 1% attended such colleges at the same rate as students with the same SAT/ACT scores from the 70th-80th percentile of the parent income distribution, calculated using equation (1) in Section 3.1. Row 6 reports the difference between Rows 2 and 5, i.e. the number of "extra" students from the top 1%. Rows 7-12 of the table decompose these extra students into the portion attributable to various parts of the admissions process. Focusing first on students who are not recruited athletes, we estimate the numbers in Row 12 (application), the sum of Rows 8 and 9 (admissions), and Row 11 (matriculation) using a calculation similar to that in Rows 5 and 6, where we set the application/admissions/ matriculation rates of top 1% students equal to those for students with the same SAT/ACT scores from the 70th-80th percentile of the income distribution. We calculate the effect of equalizing application rates on the sample from Figure 3a; we calculate the effects of equalizing admissions and matriculation rates on the sample from Figures 3b and 3c (excluding recruited athletes). In Row 8, we equalize both the share of students who are legacy applicants (Appendix Figure 10a) and the advantage such students receive in the admissions process (Appendix Figure 10b) between the top 1% and the 70th-80th percentiles. In Row 9, we remove the effects from higher non-academic ratings received by applicants from the top 1%, relative to applicants with the same test scores and academic ratings from the 70th-80th percentiles (Figure 6b). Because we estimate these constituent effects in different subsamples of the data, we align all numbers with overall Ivy-Plus averages at the end through proportional scaling. To do so, we calculate all effects in log-points and multiply by the total number of excess students in Row 6, implementing changes in the order [admissions, matriculation, application] and [legacy, non-academic ratings] within admissions. Row 10 is estimated separately from Rows 8-12 on the sample of applicants who are recruited athletes; to calculate the number in row 10, we reduce the overall fraction of students from the top 1% among recruited athletes to match the overall share of students from the top 1% in the equal attendance counterfactual in Row 3. Row 7 presents the sum of the numbers in Rows 8 through 10. See Appendix F for further details on the construction of this table.

Table 3: Predictive Power of Early-Career Outcomes for Leadership Positions
Among Ivy-Plus and Flagship Public Attendees

| | Elite Graduate School | | Prestigious Firm Employment at Age 33 | |
|---|-----------------------|--------------------|---------------------------------------|--------------------|
| | Share who Attended | Odds Ratio | Share Employed by | Odds Ratio |
| | Elite Graduate School | (R _{PS}) | Prestigious Firms | (R _{PS}) |
| | (1) | (2) | (3) | (4) |
| <i>Panel A: Income and Business</i> | | | | |
| [1] Fortune 500 CEOs | 43.5% | 5.8 | | |
| [2] Corporate Board and Committee Members | 55.2% | 9.2 | | |
| <i>Panel B: Arts and Sciences</i> | | | | |
| [3] Nobel Laureates | 89.0% | 60.7 | | |
| <i>Panel C: Public Service</i> | | | | |
| [4] Supreme Court Justices | 93.3% | 104.6 | 40.0% | 2.0 |
| [5] Treasury Secretaries | 57.1% | 10.0 | 57.1% | 4.1 |

Notes: This table analyzes how the odds of achieving various post-college leadership outcomes vary with the early-career measures of success that we use in our empirical analysis. The sample for all cells consists of individuals who attended Ivy-Plus or Flagship Public colleges. Column 1 reports the share of leaders who attended elite graduate schools (as defined in Section II), while Column 3 reports the fraction of leaders who worked at a prestigious firm (as defined in Section II). Columns 2 and 4 report odds ratios of becoming a leader conditional on achieving the corresponding early career outcome (e.g., attending an elite graduate school in Column 2) vs. not achieving that outcome. See Appendix I for further details on data sources and the construction of this table.

Table 4: Causal Effects of Attending an Ivy-Plus Instead of State Flagship College on Post-College Outcomes

| | Treatment Effect of Attending Ivy-Plus Relative to Flagship Public | | | Observed Means For Flagship Public Attendees (4) | Implied Means Had Ivy-Plus Students Attended Flagship Public (5) | Observed Means For Ivy-Plus Attendees (6) | Percentage Gain from Attending Ivy-Plus (7) |
|---|---|--------------------------------|--------------------|--|--|---|---|
| | Rescaled Waitlist Admissions Design (1) | Matriculation Design (2) | VA Estimate (3) | | | | |
| <i>Panel A: Treatment Effect on Income</i> | | | | | | | |
| Predicted Probability of Earning in Top 1% | 5.01 (1.31) | 5.14 (0.39) | 6.59 (0.01) | 8.08 | 11.81 | 16.83 | 42% |
| Predicted Probability of Earning in Top 10% | 2.49 (2.02) | 5.19 (0.91) | 5.71 (0.01) | 42.05 | 51.49 | 53.98 | 4.8% |
| Predicted Probability of Earning in Top 25% | 0.01 (1.70) | 3.06 (0.82) | 3.19 (0.01) | 65.83 | 73.33 | 73.34 | 0.0% |
| Predicted Mean Income Rank | 0.25 (0.91) | 1.78 (0.47) | 1.85 (0.00) | 76.69 | 80.38 | 80.63 | 0.3% |
| <i>Panel B: Treatment Effect on Non-Monetary Outcomes</i> | | | | | | | |
| Attend Elite Graduate School at Age 28 | 5.64 (2.79) | 2.80 (0.82) | 8.92 (0.01) | 2.65 | 6.12 | 11.76 | 92% |
| Attend Non-Elite Graduate School at Age 28 | -0.02 (0.01) | -0.02 (1.94) | -0.04 (0.01) | 13.22 | 13.83 | 13.80 | -0.2% |
| Work at Elite Firm at Age 25 | 16.96 (4.01) | 13.86 (0.82) | 23.56 (0.03) | 3.75 | 8.54 | 25.50 | 199% |
| Work at Prestigious Firm at Age 25 | 17.51 (4.26) | 13.30 (0.87) | 22.10 (0.01) | 4.05 | 7.15 | 24.66 | 245% |
| Work at Elite Firm in Occ. that Precedes Govt. Leadership | 5.49 (2.13) | 4.65 (0.31) | 7.95 (0.01) | 0.82 | 0.94 | 6.43 | 586% |
| Work at Prestigious Firm in Occ. that Precedes Govt. Leadership | 4.31 (2.06) | 4.00 (0.32) | 6.67 (0.01) | 0.82 | 1.22 | 5.53 | 354% |

Notes: This table presents regression estimates of the causal effects of attending an average Ivy-Plus college relative to the mean flagship public college (listed in Appendix Table 1). The first column shows treatment effects based on the waitlist design, calculated by multiplying the waitlist TOT effect on the relevant outcome (as estimated in Figure 8 or Figure 11) by the ratio of the difference in mean observational value-added between the Ivy-Plus and nine flagship public schools and the waitlist TOT effect on value-added of college attended (for the relevant variable). In the second column, we present estimates of the causal effects of colleges based on the matriculation design, following the approach in Figure 12b; see the notes to that figure for details. The third column shows the difference in mean observational value-added (VA) between Ivy-Plus college and flagship public colleges, where observational VA is estimated using a regression of the relevant outcome on college fixed effects and controlling for 13 bins of parental income, a quintic in SAT scores, race, gender (when available), and home state. Standard errors are reported in parentheses. Columns 4 and 6 show observed means of outcomes for flagship public and Ivy-Plus attendees in our pipeline sample, respectively. Column 5 shows the implied mean counterfactual outcome were Ivy-Plus students to attend the average flagship public college, calculated by subtracting the waitlist design causal effect estimates in Column 1 from Column 6. Column 7 reports the percentage difference between Columns 6 and 5. In Panel A, the outcome variables are an applicant's predicted likelihood of reaching the top 1%, top 10%, and top 25% based on their firm at age 22-25 and predicted income rank based on their firms at ages 22-25 (see notes to Figure 10 for details). In the first four rows of Panel B, the outcomes are indicators for attending a highly selective (elite) graduate school at age 28, a non-elite graduate school at age 28, working at an elite firm at age 25, and working at a prestigious firm at age 25. See Section 2 for definitions of these outcomes. The next two rows of Panel B define an indicator for working at an elite or prestigious firm at age 25 in an occupation that commonly precedes holding a leadership position in government. We identify such occupations by looking at the top occupations that congressional members worked in prior to Congress from the Brookings Vital Statistics on Congress data (Brookings 2022), which we map to 2-digit SOC codes. We then define the outcomes in these two rows as indicators for working at elite or prestigious firms in a (self-reported) occupation in one of these SOC codes.

Table 5: Predicted Effects of Changes in Admissions Policies on Socioeconomic Diversity and Outcomes at Ivy-Plus Colleges

Notes: Panel A of this table presents estimates of the distribution of parental incomes (Columns 1-4) and post-college outcomes (Columns 5-7) at Ivy-Plus colleges under a series of counterfactual admissions policies. In order to measure outcomes through age 28, we restrict to students who applied to college in the years 2010-2013. Row 1 presents the actual distribution of parent incomes at the 12 Ivy-Plus colleges. In Row 2, we remove legacy preferences, in two steps: first, we de-admit a fraction of legacy students based on the ratio of their modeled admissions rate as legacies to the predicted admissions rate of otherwise identical students who are not legacies; for instance, if legacy students are admitted at three times the rate of similar non-legacies, we probabilistically de-admit two of out every three legacies. We estimate the legacy and non-legacy admissions rates using our full set of admissions covariates; see the notes to Figure 5 for more details. We estimate the ratio for de-admissions separately for each group of students defined by parent-income bins and SAT/ACT score above/below 1500/34. We then re-admit students to refill the class from those either just de-admitted or those on the waitlist, using admissions rates that are proportional to students' predicted admissions rates from the non-legacy admissions model. In Row 3, we repeat this procedure (starting from the set of admitted students in Row 2) to remove the advantage to higher-income students from higher non-academic ratings. We do so in the same two steps as in Row 2: first, we de-admit students based on the ratio of their non-legacy modeled admissions rate and the admissions rate removing the admissions effect of higher non-academic ratings, and second, we re-admit students proportionally to refill the class. We estimate this effect by calculating the admissions rate were students from the top 20% to receive the same non-academic ratings as students from the 70th-80th percentiles with the same test scores and academic ratings; again, we calculate the de-admissions ratios within parent-income x SAT/ACT above/below 1500/34 x academic rating. The counterfactuals in Rows 2 and 3 leave the set of recruited athletes unchanged and work only through changing the admissions rates of non-athlete applicants. In Row 4, beginning from the distribution of admitted students in Row 3, we model a scenario where the characteristics of athletic recruits become identical to that for non-athlete admitted students by removing all recruited athletes and proportionally increasing the admission rates of non-athletes to refill the class. In Row 5, we present a separate counterfactual again beginning from the actual distribution of students in Row 1 and proportionally increasing the admissions rates of students from below the 95th percentile and with high academic ratings. Beginning with predicted admissions rates (modeled separately for legacy and non-legacy applicants, as in Appendix Figure 10b), we increase admissions rates for students from the bottom 60% and 60th to 95th percentiles of the parental income distribution by 2.3x and 1.6x respectively, chosen so that the fraction of students from these two lower income bins exactly matches the fraction in Row 4. We estimate the counterfactuals in Rows 2-5 on data from various subsets of the Ivy-Plus colleges and proportionally rescale the results to apply to the overall distribution of students across all 12 colleges. To predict counterfactual post-college outcomes, we first calculate the potential outcome for each student by subtracting the value-added for the college actually attended and adding back the mean value-added for the Ivy-Plus colleges (see Section 6); we then present the average of potential outcomes for the counterfactually admitted class. Row 6 presents a counterfactual in which all students nationally from parent income groups above the 70-80th percentiles attend Ivy-Plus colleges at rates equal to that for students with the same SAT/ACT scores but from the 70th-80th percentiles. Row 7 considers a counterfactual from Chetty et al. (2020) in which students from the bottom 20% of the income distribution are given an advantage in the college admissions process comparable in magnitude to the preference currently given to legacy applicants. In this counterfactual, Ivy-Plus attendance rates for students from the bottom 20% equal those of students with 160 point higher SAT scores from the top 20%, along with smaller boosts for students from middle-income families. See Appendix F for additional details regarding Panel A. Panel B of this table presents estimates of the increase in the share of leaders (as defined in Figure 1) whose parents are in the bottom 60% and bottom 95% of the parent income distribution under two counterfactual Ivy-Plus admissions schemes. Columns 1 and 2 present estimates under the counterfactual admissions scheme described in row 4 of Panel A. Column 3 presents estimates under the admissions scheme in row 7 of Panel A. See Appendix M for additional details on Panel B and Appendix A for definitions and sources for the leadership outcome variables.

Appendix Table 1: List of Colleges by Group

| <i>Panel A: Ivy-Plus Colleges</i> | | |
|-----------------------------------|---------------------------------------|------------------|
| No | Name | Location |
| 1 | Brown University | Providence, RI |
| 2 | Columbia University | New York, NY |
| 3 | Cornell University | Ithaca, NY |
| 4 | Dartmouth College | Hanover, NH |
| 5 | Duke University | Durham, NC |
| 6 | Harvard University | Cambridge, MA |
| 7 | Massachusetts Institute of Technology | Cambridge, MA |
| 8 | Princeton University | Princeton, NJ |
| 9 | Stanford University | Stanford, CA |
| 10 | University of Chicago | Chicago, IL |
| 11 | University of Pennsylvania | Philadelphia, PA |
| 12 | Yale University | New Haven, CT |

| <i>Panel B: Other Highly Selective Private Colleges</i> | | |
|---|------------------------------------|-----------------|
| No | Name | Location |
| 1 | California Institute of Technology | Pasadena, CA |
| 2 | Carnegie Mellon University | Pittsburgh, PA |
| 3 | Emory University | Atlanta, GA |
| 4 | Georgetown University | Washington, DC |
| 5 | Johns Hopkins University | Baltimore, MD |
| 6 | New York University | New York, NY |
| 7 | Northwestern University | Evanston, IL |
| 8 | Rice University | Houston, TX |
| 9 | University of Notre Dame | Notre Dame, IN |
| 10 | University of Southern California | Los Angeles, CA |
| 11 | Vanderbilt University | Nashville, TN |
| 12 | Washington University in St. Louis | St. Louis, MO |

| <i>Panel C: Flagship Public Colleges</i> | | |
|--|---|---------------------|
| No | Name | Location |
| 1 | The Ohio State University | Columbus, OH |
| 2 | University of California, Berkeley | Berkeley, CA |
| 3 | University of California, Los Angeles | Los Angeles, CA |
| 4 | University of Florida | Gainesville, FL |
| 5 | University of Georgia | Athens, GA |
| 6 | University of Michigan - Ann Arbor | Ann Arbor, MI |
| 7 | University of North Carolina at Chapel Hill | Chapel Hill, NC |
| 8 | University of Texas at Austin | Austin, TX |
| 9 | University of Virginia | Charlottesville, VA |

Notes: This table lists in alphabetical order the colleges within the three groups that we focus on in this paper.

Appendix Table 2: Summary Statistics by College Type, Conditional on Attendance

| | Ivy-Plus | | | Flagship Public | | | Other Selective Private | |
|--|--------------------------|---------------------------|--------------------------------|--------------------------|---------------------------|--------------------------------|--------------------------|---------------------------|
| | Pipeline Analysis (1) | Long Term Outcomes (2) | College-Specific Sample (3) | Pipeline Analysis (4) | Long Term Outcomes (5) | College-Specific Sample (6) | Pipeline Analysis (7) | Long Term Outcomes (8) |
| <i>Panel A: Standardized Test Scores</i> | | | | | | | | |
| Test Score | 1405 | 1386 | 1426 | 1211 | 1185 | 1279 | 1330 | 1299 |
| Mean Number of Scoresends | 8.42 | 7.75 | 8.23 | 6.10 | 6.05 | 6.88 | 8.43 | 7.83 |
| <i>Panel B: Demographics</i> | | | | | | | | |
| Mean Year of Birth | 1994 | 1985 | 1989 | 1994 | 1985 | 1989 | 1994 | 1985 |
| Mean Age at Matriculation | 18 | 18 | 18 | 18 | 18 | 18 | 18 | 18 |
| % Female | 48.6% | 49.0% | 51.3% | 54.1% | 54.5% | 43.5% | 51.1% | 52.3% |
| % White | 50.3% | 56.0% | 53.9% | 59.5% | 61.7% | 49.9% | 53.9% | 58.6% |
| % Black | 7.1% | 6.5% | 8.5% | 5.2% | 5.6% | 5.0% | 5.7% | 5.0% |
| % Hispanic | 10.0% | 6.6% | 8.7% | 10.5% | 7.0% | 9.4% | 9.3% | 6.5% |
| % Asian | 19.2% | 15.6% | 17.2% | 14.9% | 14.1% | 27.7% | 18.5% | 15.2% |
| % American Indian/ Native American | 0.7% | 0.6% | 1.8% | 0.3% | 0.3% | 0.6% | 0.2% | 0.3% |
| % Native Hawaiian/ Pacific Islander | 0.1% | 0.0% | 0.1% | 0.1% | 0.0% | 0.0% | 0.1% | 0.0% |
| % Unknown Race | 12.6% | 14.6% | 9.9% | 9.4% | 11.2% | 7.4% | 12.3% | 14.5% |
| <i>Panel C: Parents' Incomes</i> | | | | | | | | |
| Median Parent Household Income | \$184,356 | \$177,990 | \$183,366 | \$125,610 | \$122,355 | \$131,277 | \$181,475 | \$166,772 |
| Mean Parent Income Rank | 82.2 | 81.2 | 80.8 | 74.9 | 73.4 | 74.2 | 81.1 | 80.1 |
| <i>Panel D: Post-College Outcomes</i> | | | | | | | | |
| Median Income at Age 33 | - | \$102,267 | \$86,551 | - | \$67,174 | \$73,308 | - | \$84,482 |
| Mean Income Rank at Age 33 | - | 83.5 | 81.4 | - | 77.0 | 78.7 | - | 80.5 |
| % in Top 1% at Age 33 | - | 19.9 | 16.6 | - | 7.1 | 9.0 | - | 12.8 |
| Predicted Top 1% at 33 based on Ages 22-25 Employers | 16.8% | 15.7% | 14.8% | 8.1% | 7.0% | 8.9% | 12.1% | 10.9% |
| Predicted Income Rank at Age 33 | 80.7 | 80.0 | 79.2 | 76.7 | 75.5 | 77.1 | 78.8 | 77.9 |
| % Attending Graduate School at Age 28 | 23.7% | 26.2% | 27.0% | 15.1% | 16.2% | 17.8% | 18.2% | 19.7% |
| % Attending an Elite Graduate School at Age 28 | 10.8% | 11.9% | 12.3% | 2.5% | 2.7% | 4.1% | 4.6% | 4.7% |
| % Working at an Elite Firm | 25.5% | 25.7% | 30.4% | 3.7% | 3.6% | 5.0% | 12.8% | 12.6% |
| % Working at a Prestigious Firm | 24.7% | 25.8% | 30.8% | 4.0% | 4.0% | 5.5% | 13.8% | 14.2% |
| Number of Children | 37,352 | 89,785 | 41,212 | 123,548 | 255,705 | 387,835 | 45,047 | 94,548 |

Notes: The table replicates Panels B-F of Table 1 for subsets of students who attend each of the three groups of colleges defined in Appendix Table 1. Columns 1-3 replicate Columns 1-3 of Table 1 for students attending Ivy-Plus colleges; Columns 4-6 replicate Columns 1, 2, and 4 of Table 1 for students attending flagship public colleges, and Columns 7 and 8 replicate Column 1 and 2 of Table 1 for students attending other highly selective private colleges. See notes to Table 1 for further details.

Appendix Table 3: College Attendance and Test Score Distributions by Parent Income

| | Parent Income Percentile (in National Distribution) | | | | | | | | | | | | | | Share of Ivy-Plus Attendees (17) | | |
|--|---|--------------|--------------|--------------|--------------|--------------|--------------|--------------|--------------|---------------|---------------|---------------|---------------|---------------|--|------------------|--------|
| | 0-10 (1) | 10-20 (2) | 20-30 (3) | 30-40 (4) | 40-50 (5) | 50-60 (6) | 60-70 (7) | 70-80 (8) | 80-90 (9) | 90-95 (10) | 95-96 (11) | 96-97 (12) | 97-98 (13) | 98-99 (14) | 99-99.9 (15) | Top 0.1% (16) | |
| Panel A: Parental Income Distributions | | | | | | | | | | | | | | | | | |
| P(Parent Income in Given Range Attend) | | | | | | | | | | | | | | | | | |
| Parental Income Distribution of Ivy-Plus Students | 1.4% | 1.8% | 2.1% | 2.6% | 3.3% | 4.6% | 5.7% | 8.2% | 14.9% | 13.6% | 4.2% | 5.3% | 7.1% | 9.2% | 12.7% | 3.1% | 100.0% |
| Parental Income Distribution of Flagship Public Students | 1.9% | 2.9% | 3.4% | 4.0% | 5.1% | 6.6% | 8.3% | 11.4% | 18.2% | 14.7% | 3.9% | 4.3% | 4.8% | 5.1% | 4.8% | 0.5% | 0.0% |
| Panel B: Distribution of Test Scores Conditional on Parent Income | | | | | | | | | | | | | | | | | |
| P(Test Score in Given Range Parent Income Group) | | | | | | | | | | | | | | | | | |
| 1500-1600 (or ACT of 34 - 36) | 0.0% | 0.0% | 0.1% | 0.1% | 0.1% | 0.2% | 0.3% | 0.4% | 0.8% | 1.7% | 2.5% | 2.8% | 3.4% | 3.9% | 4.6% | 6.8% | 32.6% |
| 1400-1490 (or ACT of 32 - 33) | 0.1% | 0.1% | 0.2% | 0.2% | 0.3% | 0.5% | 0.8% | 1.3% | 2.2% | 3.8% | 5.0% | 5.6% | 6.4% | 7.3% | 8.6% | 11.6% | 27.0% |
| 1300-1390 (or ACT of 29 - 31) | 0.4% | 0.4% | 0.6% | 0.7% | 1.1% | 1.7% | 2.4% | 3.6% | 5.6% | 8.3% | 10.5% | 11.3% | 12.1% | 13.1% | 14.3% | 14.7% | 20.9% |
| 1200-1290 (or ACT of 27 - 28) | 0.7% | 0.8% | 1.1% | 1.4% | 2.0% | 2.9% | 4.0% | 5.7% | 8.3% | 11.4% | 13.2% | 14.0% | 14.5% | 15.2% | 15.6% | 14.7% | 11.1% |
| 1100-1190 (or ACT of 24 - 26) | 1.6% | 2.0% | 2.5% | 3.1% | 4.3% | 6.0% | 8.1% | 10.7% | 14.1% | 17.1% | 18.2% | 18.5% | 18.8% | 19.1% | 18.4% | 16.2% | 6.0% |
| 1000-1090 (or ACT of 22 - 23) | 2.3% | 2.9% | 3.5% | 4.3% | 5.7% | 7.4% | 9.4% | 11.6% | 13.9% | 15.2% | 15.1% | 14.8% | 14.4% | 13.6% | 12.7% | 10.6% | 1.7% |
| 900-990 (or ACT of 19 - 21) | 3.8% | 5.2% | 6.1% | 7.2% | 8.7% | 10.4% | 12.3% | 14.0% | 14.7% | 14.0% | 12.8% | 12.2% | 11.4% | 10.6% | 9.6% | 8.3% | 0.6% |
| 800-890 (or ACT of 17 - 18) | 3.9% | 5.6% | 6.4% | 7.1% | 7.8% | 8.3% | 8.8% | 9.0% | 8.6% | 7.3% | 6.3% | 5.7% | 5.3% | 4.7% | 4.2% | 3.8% | 0.1% |
| 700-790 (or ACT of 15 - 16) | 3.6% | 5.4% | 5.8% | 6.0% | 5.9% | 5.7% | 5.3% | 4.8% | 4.1% | 3.0% | 2.4% | 2.2% | 2.0% | 1.8% | 1.5% | 1.3% | 0.0% |
| 600-690 (or ACT of 13 - 14) | 2.5% | 3.7% | 3.8% | 3.7% | 3.3% | 2.9% | 2.4% | 2.0% | 1.5% | 1.0% | 0.8% | 0.7% | 0.6% | 0.6% | 0.5% | 0.4% | 0.0% |
| below 600 (or ACT below 12) | 1.2% | 1.8% | 1.7% | 1.6% | 1.4% | 1.0% | 0.8% | 0.6% | 0.4% | 0.3% | 0.2% | 0.2% | 0.2% | 0.2% | 0.2% | 0.2% | 0.0% |
| Did not take SAT or ACT | 79.8% | 71.9% | 68.3% | 64.5% | 59.5% | 53.1% | 45.6% | 36.4% | 25.8% | 16.9% | 13.2% | 12.2% | 11.1% | 10.3% | 10.3% | 12.1% | 0.0% |

Notes: This table presents statistics on parental income distributions for Ivy-Plus vs. flagship public attendees (Panel A) and the distribution of SAT/ACT scores by parental income (Panel B). Parent income percentile is defined as parent income rank relative to other parents with children in the same birth cohort in the full national sample from tax records. The first row of Panel A presents the share of all 12 Ivy-Plus college students coming from each parent income group. The second row of Panel A replicates the same statistics for students who attend one of the flagship public colleges listed in Appendix Table 1. Panel B presents the distribution of SAT scores (or ACT score equivalents) by parent income percentile, showing the fraction who took the test and scored in the range listed in each row conditional on having parent income in the group listed in the relevant column (including non-test-takers in the denominator when calculating fractions). The last row shows the fraction of students in each parent's income bin who did not take either the SAT or ACT. The last column of Panel B shows the share of Ivy-Plus students with test scores in each of the groups listed in the rows. The shares in each column of Panel B sum to 100%. Both panels are based on our pipeline analysis sample.

Appendix Table 4: Parent and Child Income Distributions

| <i>Panel A. Quantiles</i> | | | |
|---------------------------|--|--|---|
| Percentile | Parents' Household Income When Child is Aged 12-17, 1991-1996 Cohorts | Parents' Household Income When Child is Aged 12-17, 1982-1988 Cohorts | Children's Individual Income at Age 33 |
| (1) | (2) | (3) | (4) |
| 10 | \$11,725 | \$14,400 | \$0 |
| 20 | \$19,000 | \$23,400 | \$0 |
| 30 | \$26,600 | \$32,900 | \$5,400 |
| 40 | \$35,700 | \$44,200 | \$17,500 |
| 50 | \$47,700 | \$57,800 | \$27,000 |
| 60 | \$63,600 | \$73,200 | \$36,200 |
| 70 | \$83,700 | \$91,100 | \$46,700 |
| 80 | \$110,600 | \$114,400 | \$61,000 |
| 90 | \$160,300 | \$158,200 | \$88,300 |
| 95 | \$230,200 | \$222,400 | \$120,600 |
| 96 | \$261,300 | \$251,100 | \$133,100 |
| 97 | \$311,200 | \$296,500 | \$151,600 |
| 98 | \$404,100 | \$380,000 | \$183,200 |
| 99 | \$633,800 | \$611,400 | \$261,000 |
| Top 0.1 | \$2,992,100 | \$2,662,300 | \$879,100 |

| <i>Panel B. Averages</i> | | | |
|--------------------------|-------------|-------------|--|
| 0-10 | \$6,000 | \$8,100 | |
| 10-20 | \$15,400 | \$1,900 | |
| 20-30 | \$22,700 | \$28,100 | |
| 30-40 | \$31,000 | \$38,400 | |
| 40-50 | \$41,400 | \$50,900 | |
| 50-60 | \$55,300 | \$65,400 | |
| 60-70 | \$73,200 | \$81,900 | |
| 70-80 | \$96,300 | \$102,100 | |
| 80-90 | \$131,700 | \$132,900 | |
| 90-95 | \$188,700 | \$184,100 | |
| 95-96 | \$244,700 | \$235,800 | |
| 96-97 | \$284,000 | \$271,930 | |
| 97-98 | \$352,100 | \$333,400 | |
| 98-99 | \$497,600 | \$464,600 | |
| 99-99.9 | \$1,125,900 | \$1,030,000 | |
| Above Top 0.1 | \$8,717,000 | \$6,699,500 | |

Notes: This table lists the dollar amounts for parental household income when children are aged 12-17 and children's individual income at age 33. Panel A lists the amounts at specific quantiles of the national income distribution. Panel B lists the average within each bin of the national income distribution. Column 2 averages over the 1991-1996 birth cohorts, columns 2 and 3 average over the 1982-1988 birth cohorts. Children's income percentiles are constructed by ranking children relative to all other children in the same birth cohort and parents' percentiles are defined by ranking parents relative to all other parents with children in the same birth cohort. See Section 2 for details on income definitions. All monetary values are in 2015 dollars.

Appendix Table 5: Excess Ivy-Plus Attendees due to Differences in Admissions Rates
 Conditional on Test Scores, by Parental Income

| Parent Income Percentile | Excess Students | |
|-----------------------------|-----------------------------------|----------------------------|
| | Relative to "Middle Class" (1) | Relative to Average (2) |
| 0-20 | 16 | 10 |
| 20-40 | 25 | 18 |
| 40-60 | 31 | 17 |
| 60-70 | 14 | 2 |
| 70-80 | 0 | -19 |
| 80-90 | -6 | -42 |
| 90-95 | -16 | -51 |
| 95-96 | 2 | -8 |
| 96-97 | 3 | -10 |
| 97-98 | 16 | 1 |
| 98-99 | 26 | 8 |
| 99-99.9 | 65 | 44 |
| Top .1 | 31 | 28 |

Notes: This table presents estimates of the excess number of students attending the average Ivy-Plus institution due to differences in admissions rates conditional on test scores (excluding athletes). Column 1 presents estimates relative to the admissions rate for students whose parents have incomes in the 70-80th percentile of the parent income distribution, calculated in the same way as in Table 2. Column 2 presents estimates relative to the average admissions rate (pooling all percentiles, rather than just those between the 70th-80th percentile). The numbers in this table do not exactly match those in Table 2 due to differences in the coarseness of SAT bins used in this supplementary analysis.

Appendix Table 6: Additional Students from Top 1% at Ivy-Plus Colleges: Simultaneous Decomposition Analysis

| | | Total | Total | Subtotal | Share of Excess Top 1% Students |
|------|---|-------|-------|----------|---------------------------------|
| [1] | Class Size | 1650 | 1650 | | |
| [2] | Total Students with Parent Income in Top 1% | 261 | 261 | | |
| [3] | Top 1% Students Given Equal Attendance Cond. on Test Scores | 93 | 93 | | |
| [4] | Total Excess Students with Parent Income in Top 1% | 168 | 168 | | 100.0% |
| [5] | Attributable to Application Rates | 49 | 49 | | 29.1% |
| [6] | Attributable to Admission Rates | 69 | 96 | | 57.6% |
| [7] | Legacy | 41 | | 41 | 24.7% |
| [8] | Ratings | 28 | | 28 | 16.7% |
| [9] | Athletes | 27 | | 27 | 16.2% |
| [10] | Attributable to Matriculation Rates | 22 | 23 | | 13.3% |

Notes: This table replicates Table 2 (excluding rows 3 and 4 of that table), except that we report statistics that average over the different possible orderings of policy changes (application, admission, matriculation) rather than prioritizing admissions. To do so, we proportionally allocate students across the stages of the pipeline based on the ratio of the log-point difference in attendance rates from each stage (application, admission, and matriculation) and the total log-point difference. See Table 2 and Appendix F for details.

Appendix Table 7: Waitlist Design: Balance Tests

| | Waitlist Reject | | Waitlist Admit | | Difference (5) | SE (6) | Difference as % of Non-Admit (7) | P-Value (8) |
|--|-----------------|-----------|----------------|-----------|-------------------|-----------|--|----------------|
| | Mean (1) | SD (2) | Mean (3) | SD (4) | | | | |
| <i>Panel A: Predicted Outcomes</i> | | | | | | | | |
| Placebo Predicted Top 1% at 33 based on Ages 22-25 Employers | 14.27 | 4.65 | 14.16 | 4.34 | -0.11 | 0.14 | -2.33% | 0.43 |
| Placebo % Attending Graduate School at Age 28 | 10.76 | 4.53 | 10.74 | 4.46 | -0.02 | 0.17 | -0.37% | 0.92 |
| <i>Panel B: Demographics</i> | | | | | | | | |
| % Female | 52.09 | 49.96 | 52.08 | 49.84 | -0.01 | 1.46 | -0.02% | 0.99 |
| % Underrepresented Minority | 13.06 | 33.69 | 14.09 | 34.92 | 1.03 | 0.94 | 3.06% | 0.27 |
| % First-Generation College Student | 9.47 | 29.24 | 8.45 | 28.99 | -1.01 | 0.87 | -3.46% | 0.24 |
| <i>Panel C: Academic Credentials</i> | | | | | | | | |
| Test Score | 1450.27 | 107.31 | 1442.52 | 111.42 | -7.75 | 3.05 | -7.22% | 0.01 |
| High School Grade Point Average | 3.86 | 0.18 | 3.84 | 0.18 | -0.02 | 0.01 | -9.84% | 0.13 |
| <i>Panel D: High School Quality and College Applications</i> | | | | | | | | |
| Predicted Top 1% at 33 based on HS FE on Admissions | 15.86 | 9.02 | 16.43 | 8.32 | 0.58 | 0.35 | 6.39% | 0.10 |
| Number of Standardized Test Score Sends | 10.66 | 4.27 | 10.71 | 4.12 | 0.05 | 0.16 | 1.24% | 0.73 |
| <i>Panel E: Parent Income and Legacy Status</i> | | | | | | | | |
| % With Parent Income between 90th-95th Percentile | 15.20 | 35.91 | 15.39 | 35.97 | 0.19 | 1.06 | 0.53% | 0.86 |
| % With Parent Income between 95th-99th Percentile | 25.62 | 43.66 | 26.70 | 44.11 | 1.07 | 1.29 | 2.45% | 0.40 |
| % With Parent Income in Top 1% | 12.03 | 32.54 | 18.49 | 38.64 | 6.46 | 1.10 | 19.86% | 0.00 |
| % Legacy | 6.14 | 24.02 | 14.04 | 34.65 | 7.90 | 1.02 | 32.87% | 0.00 |

Notes: This table compares the application characteristics of Ivy-plus applicants who were accepted vs. rejected after being placed on the waitlist. The sample consists of all applicants in our college-specific sample who were offered a place on the waitlist at an Ivy-Plus college. To construct Columns 1 and 3, we regress the variable listed in the relevant row on an indicator for admission and fixed effects for the college at which the applicant is waitlisted. We weight applicants such that the number of matriculants is the same for each college in our sample. We calculate the waitlist reject mean as the overall waitlist mean minus the coefficient on the indicator for admissions times the share of students admitted. The waitlist admit mean is the waitlist reject mean plus the coefficient on admissions from the regression. Columns 2 and 4 report the standard deviation of each variable, separately for students rejected and admitted from the waitlist. Columns 5-8 report the coefficient on the indicator for admission, the standard error of the coefficient (clustered by student to account for students who were waitlisted at multiple schools), that coefficient as percentage of the standard deviation of the relevant variable among students rejected from the waitlist, and a p value for the null hypothesis that the difference is 0. The placebo outcomes in Panel A are predicted values from regressions of outcome variables on indicators for legacy status, parent income bin, the full tuple of admissions office ratings, gender, home state, ethnicity, and recruited athlete status, a quintic in SAT, and the college to which the student applied. In Panel D, the first variable is the predicted value from a regression of the predicted top 1% variable on high school fixed effects, and the second variable is the number of colleges to which each student sends standardized test scores. See Section 2 for other variable definitions.

Appendix Table 8: Waitlist Design Treatment Effect Estimates

| | Raw Means | | With Controls | | Observational Value-Added | |
|---|-----------------|-----------------------|-----------------|-----------------------|---------------------------|-----------------------|
| | Pooled (1) | Non-Advantaged (2) | Pooled (3) | Non-Advantaged (4) | Pooled (5) | Non-Advantaged (6) |
| <i>Panel A: Income at Age 33</i> | | | | | | |
| Actual Top 1% | 5.01 (2.40) | 5.98 (2.92) | 3.97 (2.38) | 5.68 (2.92) | 3.17 (0.19) | 3.54 (0.25) |
| Fraction Earning No Income | 0.98 (0.54) | 1.32 (0.69) | 0.84 (0.50) | 1.03 (0.64) | 0.03 (0.02) | 0.03 (0.03) |
| Actual Mean Income Rank | -0.67 (1.50) | -1.14 (1.89) | -0.92 (1.47) | -1.00 (1.82) | 0.78 (0.13) | 0.93 (0.17) |
| Log(Income), Restricting to Positive Earnings | 0.15 (0.07) | 0.19 (0.08) | 0.09 (0.07) | 0.14 (0.08) | 0.09 (0.01) | 0.10 (0.01) |
| Log(Wage Earnings) | 0.07 (0.06) | 0.08 (0.07) | 0.04 (0.05) | 0.06 (0.07) | 0.07 (0.01) | 0.07 (0.01) |
| <i>Panel B: Predicted Outcomes Based on Employers at Ages 22-25</i> | | | | | | |
| Predicted Top 1% Probability | 2.72 (0.71) | 2.20 (0.81) | 2.89 (0.69) | 2.51 (0.78) | 3.57 (0.08) | 3.69 (0.10) |
| Predicted Top 10% Probability | 1.13 (0.92) | 0.65 (1.09) | 1.74 (0.89) | 1.15 (1.05) | 2.59 (0.11) | 2.65 (0.14) |
| Predicted Top 25% Probability | 0.00 (0.71) | -0.42 (0.87) | 0.40 (0.71) | -0.27 (0.85) | 1.33 (0.08) | 1.36 (0.10) |
| Predicted Mean Income Rank | 0.12 (0.45) | -0.31 (0.54) | 0.35 (0.44) | -0.19 (0.53) | 0.91 (0.05) | 0.93 (0.06) |
| <i>Panel C: Non-Monetary Outcomes</i> | | | | | | |
| Attend Elite Graduate School at Age 28 | 3.17 (1.57) | 3.84 (1.92) | 3.23 (1.59) | 4.57 (1.95) | 5.01 (0.10) | 5.18 (0.13) |
| Attend Non-Elite Graduate School at Age 28 | 1.31 (1.70) | -0.01 (1.97) | 1.18 (1.71) | -0.37 (1.97) | -0.83 (0.08) | -0.81 (0.10) |
| Work at Elite Firm at Age 25 | 9.08 (2.15) | 9.23 (2.54) | 8.41 (2.17) | 8.63 (2.55) | 12.61 (0.25) | 12.86 (0.31) |
| Work at Prestigious Firm at Age 25 | 8.85 (2.15) | 10.57 (2.58) | 7.94 (2.18) | 9.60 (2.61) | 11.17 (0.18) | 11.42 (0.23) |

Notes: This table presents estimates of the causal effect of attending an Ivy-plus college in our college-specific sample using the waitlist design, with standard errors (clustered by student) in parentheses. The estimates in Columns 1 and 3 are constructed using the same approach as those reported in Figures 8 and 11; see notes to those figures for details. The estimates in Columns 2, 4, and 6 replicate estimates in Columns 1, 3, and 5 respectively except excluding legacy applicants, recruited athletes, and applicants with parents in the top 1% of the income distribution. Columns 5 and 6 report treatment effects on the quality of college attended, as measured by the college's observational value-added on the relevant outcome. Observational value-added estimates are based on OLS regressions of outcomes on fixed effects for the college students attend, controlling for parental income, SAT scores, race, gender, birth cohort, and home state, estimated using our pipeline analysis sample. In Panel A, we report estimates for actual earnings outcomes at age 33. In row 1, the outcome is an indicator for having earnings in the top 1% at age 33. Row 2 defines the outcome as the share earning no income at age 33. Row 3 presents estimates on mean income rank. In row 4, the outcome is log income in 2015 dollars, restricting to individuals with positive income. Row 5 presents estimates for wage earnings, defined as W-2 earnings plus self-employment earnings for single filers or W-2 earnings plus self-employment earnings plus half of the difference between total wage earnings and self-employment reported on the Form 1040 and the sum of both filers' W-2 and Form SE earnings for joint filers. In row 5, we further restrict to earnings above \$15,800 to match the restriction imposed by Dale and Krueger (2014). Panel B presents estimates for predicted outcomes based on employers at ages 22-25 and Panel C presents estimates for non-monetary outcomes. See Section 2 for definitions of variables in Panels B and C.

Appendix Table 9: Heterogeneity in Treatment Effects of Ivy-Plus Admission by Strength of Outside Options

| | | Predicted Top 1% | | | | | | |
|--|--|-------------------------|----------------|---------------------|--------------------------------------|------------------------------|---|----------------|
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Observational VA of College Attended | | 0.79 (0.20) | 0.82 (0.32) | 0.83 (0.29) | 0.87 (0.20) | 0.87 (0.27) | 0.93 (0.22) | 0.69 (0.21) |
| Implied effect of attending Ivy-Plus Instead of Flagship Public (pp) | | 5.14 (1.29) | 5.34 (2.09) | 5.39 (1.88) | 5.64 (1.31) | 5.68 (1.79) | 6.07 (1.45) | 4.50 (1.39) |
| Grouping Instrument Construction | Baseline: Homestate, Race, Income School Applied | Baseline with Jackknife | CZ Only | Flexible Regression | Constructed on Regular Reject Sample | Dropping Multi-Campus Groups | Baseline, but dropping FE for Ivy-plus college to which student applied | |

Notes: This table presents estimates of how the causal effect of being admitted to an Ivy-Plus college on a student's predicted chances of reaching the top 1% (based on age 22-25 firms) varies with the student's outside options. The first row of each column reports the coefficient from a 2SLS regression of the predicted top 1% outcome on the observational VA of the college the individual attends, instrumenting for observational VA with the interaction between an admissions indicator and the gain in observational VA from Ivy-plus admission relative to the mean outside option for that group of students (gj). We also control directly for the admissions indicator, gj, and indicators for the Ivy-Plus college to which the student applied. The regression is estimated using the sample of students waitlisted at Ivy-Plus colleges in our college-specific sample. Standard errors are clustered by student, and reported in parentheses. The second row reports the implied causal effect of attending the average Ivy-Plus college instead of the average state flagship public college listed in Appendix Table 1, estimated as the regression coefficient multiplied by the difference in observational VA between the average Ivy-plus college and the average highly selective state flagship. In Column 1, we divide students into groups g based on their home state, race, parent income, and Ivy-Plus college applied to, and estimate their outside option as the mean observational value-added of the colleges that students rejected from the waitlist in their group attend, as in Figure 10. Columns 2-7 present alternative estimates using different methods of constructing students' outside options. In Column 2, we leave out the own student when calculating the average value-added of the outside option among students rejected from the waitlist. In Column 3, we group students based only on their commuting zone (CZ) of residence, as measured in the tax data. Column 4 predicts the outside option using an OLS regression (estimated among waitlist rejects) of observational VA on the following controls: college attended interacted with the school year, parent income bin, race, dummies for test scores, home state, and gender. Column 5 estimates the outside options using the approach in Column 1, but using the pool of rejected applicants not offered a place on the waitlist. Column 6 omits large multi-campus groups for which we cannot estimate college-specific value-added of outside options (see Chetty et al. 2020 for more details on this issue). Column 7 uses the same outside option definition as in Column 1 but drops the fixed effect for the college on which a student is on the waitlist from the regression specification so that the variation in the instrument comes both from outside options and differences in observational VA across the Ivy-plus colleges in our college-specific sample.

Appendix Table 10: Heterogeneity in Causal Effects of Ivy-Plus Attendance on Predicted Top 1% Earnings Probability

| | Effects on Predicted Top 1% Probability | |
|--|---|--------------------------|
| | Waitlist Design | Matriculation Design |
| <i>Panel A: Pooled Sample Estimate</i> | | |
| | (1.00) 2.72 (0.71) | (2.00) 5.14 (0.39) |
| <i>Panel B: Heterogeneity by Parental Income</i> | | |
| P0-P60 | 1.28 (1.43) | 3.63 (0.94) |
| P60-P95 | 3.20 (1.08) | 3.31 (0.66) |
| P95-P99 | 1.06 (1.19) | 5.05 (1.09) |
| Top 1% | 2.77 (1.82) | 6.94 (4.57) |
| P-Value from F-test of null of no heterogeneity | 0.60 | 0.00 |
| <i>Panel C: Heterogeneity by Test Score</i> | | |
| < 1300 | 3.34 (2.82) | 2.31 (0.84) |
| 1300-1400 | 1.97 (1.18) | 3.61 (1.65) |
| 1400-1500 | 1.63 (0.98) | 2.83 (5.11) |
| 1500-1600 | 4.60 (1.30) | 4.92 (16.65) |
| P-Value from F-test of null of no heterogeneity | 0.24 | 0.00 |
| <i>Panel D: Heterogeneity by Academic Rating</i> | | |
| High Academic Rating | 3.40 (1.68) | |
| Low Academic Rating | 3.42 (2.11) | |
| P-Value from F-test of null of no heterogeneity | 0.99 | |
| <i>Panel E: Heterogeneity by Athlete Status</i> | | |
| Athlete | 6.37 (5.58) | |
| Non-Athlete | 2.49 (0.67) | |
| P-Value from F-test of null of no heterogeneity | 0.49 | |
| <i>Panel F: Heterogeneity by Non-Academic Rating</i> | | |
| High Non-Academic Rating | 3.40 (1.68) | |
| Low Non-Academic Rating | 3.42 (2.11) | |
| P-Value from F-test of null of no heterogeneity | 0.99 | |
| <i>Panel G: Heterogeneity by Legacy Status</i> | | |
| Legacy | 5.95 (1.60) | |
| Non-Legacy | 2.25 (0.74) | |
| P-Value from F-test of null of no heterogeneity | 0.04 | |

Notes: This table presents estimates of the causal effects of attending an Ivy-Plus college on an applicant's predicted likelihood of reaching the top 1% based on their firms at ages 22-25, separately by student characteristics. Column 1 presents estimates using the waitlist design, following the estimator in the first pair of bars in Figure 8b; see notes to that figure for details. Column 2 presents estimates using the matriculation design, following the approach in Figure 12b; again, see notes to that figure for details. Panel A shows estimates for the full sample, while Panels B-G report estimates among subgroups of applicants with certain observable characteristics. The last row of each panel reports p-values for the null hypothesis of homogeneous treatment effects across the subgroups shown in each panel.

Appendix Table 11: Distribution of Earnings at Age 33 by College Type

| Percentile | Ivy-Plus | Unweighted Flagship Public | Reweighted Flagship Public | Unweighted Other Highly Selective Private | Reweighted Other Highly Selective Private |
|------------|--------------|-------------------------------|-------------------------------|--|--|
| (1) | (2) | (3) | (4) | (5) | (6) |
| 20 | \$47,900 | \$33,600 | \$40,000 | \$39,800 | \$43,000 |
| 40 | \$83,500 | \$56,600 | \$69,600 | \$70,900 | \$74,800 |
| 60 | \$134,100 | \$83,900 | \$106,700 | \$107,800 | \$115,100 |
| 70 | \$173,000 | \$103,100 | \$133,300 | \$135,700 | \$146,100 |
| 80 | \$235,500 | \$131,600 | \$177,800 | \$179,600 | \$195,000 |
| 90 | \$365,600 | \$194,600 | \$269,700 | \$271,700 | \$292,600 |
| 95 | \$555,900 | \$280,600 | \$377,900 | \$381,600 | \$407,700 |
| 96 | \$649,200 | \$311,800 | \$416,600 | \$426,000 | \$452,000 |
| 97 | \$796,900 | \$355,400 | \$479,500 | \$496,300 | \$518,800 |
| 98 | \$1,096,400 | \$422,000 | \$622,600 | \$625,700 | \$662,400 |
| 99 | \$1,789,600 | \$605,800 | \$927,000 | \$982,400 | \$1,028,500 |
| Top 0.1 | \$11,363,200 | \$2,821,600 | \$3,993,600 | \$5,475,700 | \$5,271,200 |

Notes: This table presents quantiles of the distributions of earnings at age 33 for individuals who attended Ivy-Plus, Flagship Public, or Other Highly Selective Private colleges (listed in Appendix Table 1). Column 2 reports the distribution of earnings for individuals who attended Ivy-Plus colleges. Columns 3 and 5 report distributions of earnings among students who attended flagship public and other selective private colleges, respectively. Columns 4 and 6 replicate columns 3 and 5 after reweighting on parent income bin, gender, race, and test score to match Ivy-Plus matriculants. The sample consists of all students in our long-term outcomes sample for whom we observe college attendance, SAT/ACT scores, and income at age 33.

Appendix Table 12: Observed Shares Above Upper-Tail Income Thresholds vs. Predictions Based on Log-Constant Model

| | 90th Percentile (Nationally) \$106,611 (1) | 99th Percentile (Nationally) \$241,075 (2) | 95th Percentile (Ivy-Plus) \$555,900 (3) | 99th Percentile (Ivy-Plus) \$1,789,600 (4) |
|--|---|---|---|---|
| [1] Observed Ivy-Plus Students | 49.6% | 19.4% | 5.0% | 1.0% |
| [2] Observed Flagship Public Students Reweighted on Test Score, Race, Gender, and Parent Income | 40.0% | 12.3% | 2.4% | 0.4% |
| [3] Actual Ivy-Plus Treatment Effect | 9.6% | 7.1% | 2.6% | 0.6% |
| [4] Predicted Ivy-Plus Outcomes Assuming Log-Constant (23%) Treatment Effect | 51.0% | 17.9% | 3.6% | 0.5% |
| [5] Predicted Log-Constant Treatment Effect | 11.0% | 5.6% | 1.2% | 0.1% |
| [6] Ratio of Actual to Predicted Log-Constant Treatment Effect | 0.78 | 1.27 | 2.17 | 6.00 |

Notes: This table presents estimates of the share of students with incomes at age 33 above various income thresholds using the same sample as Appendix Figure 26. In Columns 1 and 2, the thresholds are the 90th and 99th percentile of the national income distribution at age 33; in Columns 3 and 4, the thresholds are the 95th and 99th percentiles of the income distributions at age 33 among Ivy-Plus attendees. Dollar values (in 2015 dollars) corresponding to these thresholds are listed in each column. Row 1 reports the actual fraction of students from Ivy-Plus students reaching each threshold (corresponding to the green series in Appendix Figure 26). Row 2 reports the fraction of students reaching those thresholds from flagship public colleges, reweighted to match Ivy-Plus students on test score, race, gender, and parent income (yellow series in Appendix Figure 26). The difference between these values is the Ivy-Plus treatment effect for the given income threshold, reported in Row 3. Rows 4-6 then compare the actual treatment effect to that which would result from a log-constant treatment effect. We estimate the log-constant treatment effect as the unweighted average difference between the log of the inverse CDF of the income distribution for Ivy-Plus students and that for flagship public students (reweighted on test scores and demographics) at the points 0.01 through 0.99, yielding an average log increase of 23%. Row 4 reports counterfactual estimates of shares of Ivy-Plus students above each income threshold if we increase each quantile of the reweighted distribution for students from highly selected public colleges by 23%. The difference between Row 4 and Row 2 is the treatment effect that would result at each income threshold from a 23% proportional increase in the reweighted flagship public college income distribution. Row 6 reports the ratio of Row 2 to Row 5, i.e., the ratio of the actual observed treatment effect at each threshold to the predicted treatment effect under a log-constant proportional shift.

Appendix Table 13: Association Between Ivy-Plus Students' Post-College Outcomes and ACT/SAT Scores vs. High School GPA

| | (1) | (2) | (3) | (4) | (5) | (6) |
|--|------------------|------------------|-------------------|-------------------|-------------------|-------------------|
| <i>Panel A: Predicted Top 1% Earnings Probability</i> | | | | | | |
| SAT (or converted ACT) Percentile | 0.107 (0.008) | | 0.108 (0.008) | 0.095 (0.009) | 0.094 (0.009) | 0.059 (0.014) |
| High School GPA Percentile | | 0.018 (0.009) | -0.002 (0.009) | 0.010 (0.009) | 0.010 (0.010) | 0.056 (0.020) |
| Legacy | | | | -2.831 (0.676) | -2.737 (0.684) | -2.718 (0.925) |
| Recruited Athlete | | | | 1.358 (0.742) | 1.295 (0.751) | 0.936 (1.155) |
| R ² | 0.025 | 0.001 | 0.025 | 0.049 | 0.069 | 0.297 |
| Mean of Dependent Variable | 19.430 | 19.430 | 19.430 | 19.430 | 19.439 | 20.149 |
| Implied Difference Between SAT Score of 1400 and 1600 | 7.612 | | 7.637 | 6.712 | 6.651 | 4.204 |
| Implied Difference Between GPA of 3.75 and 4.0 | | 0.904 | -0.120 | 0.495 | 0.523 | 2.860 |
| Race, Gender, and Parent Income FEs | No | No | No | Yes | Yes | Yes |
| Race x Gender x Parent Income FEs | No | No | No | No | Yes | Yes |
| High School FE | No | No | No | No | No | Yes |
| Number of Observations | 7,081 | 7,081 | 7,081 | 7,081 | 7,070 | 5,282 |
| <i>Panel B: Elite Graduate School Attendance at Age 25</i> | | | | | | |
| SAT (or converted ACT) Percentile | 0.149 (0.015) | | 0.142 (0.016) | 0.135 (0.018) | 0.136 (0.019) | 0.095 (0.029) |
| High School GPA Percentile | | 0.072 (0.017) | 0.046 (0.017) | 0.032 (0.018) | 0.032 (0.018) | 0.133 (0.037) |
| Legacy | | | | -2.280 (1.336) | -2.127 (1.351) | -2.421 (1.800) |
| Recruited Athlete | | | | -4.671 (1.252) | -4.331 (1.269) | -0.566 (1.955) |
| R ² | 0.013 | 0.002 | 0.014 | 0.028 | 0.047 | 0.267 |
| Mean of Dependent Variable | 15.718 | 15.718 | 15.718 | 15.718 | 15.700 | 16.073 |
| Implied Difference Between SAT Score of 1400 and 1600 | 10.577 | | 10.107 | 9.563 | 9.668 | 6.770 |
| Implied Difference Between GPA of 3.75 and 4.0 | | 3.693 | 2.338 | 1.649 | 1.651 | 6.761 |
| Race, Gender, and Parent Income FEs | No | No | No | Yes | Yes | Yes |
| Race x Gender x Parent Income FEs | No | No | No | No | Yes | Yes |
| High School FE | No | No | No | No | No | Yes |
| Number of Observations | 7,081 | 7,081 | 7,081 | 7,081 | 7,070 | 5,282 |
| <i>Panel C: Employment at Prestigious Firm at Age 25</i> | | | | | | |
| SAT (or converted ACT) Percentile | 0.212 (0.024) | | 0.207 (0.024) | 0.155 (0.029) | 0.159 (0.030) | 0.131 (0.046) |
| High School GPA Percentile | | 0.075 (0.027) | 0.037 (0.027) | 0.031 (0.028) | 0.032 (0.029) | 0.156 (0.063) |
| Legacy | | | | -1.000 (2.129) | -0.906 (2.172) | -3.729 (2.947) |
| Recruited Athlete | | | | -7.331 (2.186) | -7.212 (2.218) | -5.138 (3.610) |
| R ² | 0.017 | 0.000 | 0.017 | 0.034 | 0.051 | 0.272 |
| Mean of Dependent Variable | 16.116 | 16.116 | 16.116 | 16.116 | 16.123 | 16.574 |
| Implied Difference Between SAT Score of 1400 and 1600 | 5.540 | | 5.593 | 4.836 | 4.740 | 3.039 |
| Implied Difference Between GPA of 3.75 and 4.0 | | 0.486 | -0.264 | 0.185 | 0.228 | 2.075 |
| Race, Gender, and Parent Income FEs | No | No | No | Yes | Yes | Yes |
| Race x Gender x Parent Income FEs | No | No | No | No | Yes | Yes |
| High School FE | No | No | No | No | No | Yes |
| Number of Observations | 7,081 | 7,081 | 7,081 | 7,081 | 7,070 | 5,282 |

Notes: This table presents OLS regression estimates of students' post-college outcomes on various student characteristics at the time of application. In each panel, each column presents results from a single regression on the variables listed in that column. SAT (or converted ACT) scores and high school grade point average (GPA) are converted to percentile ranks among Ivy-plus attendees so that the coefficients on those variables can be interpreted as the effect of a 1 percentile increase in the explanatory variable on the outcome. The dependent variable in Panel A is the predicted top 1% share based on firms at ages 22-25 (see Section 2 for more details); in Panels B and C, it is an indicator variable for attending an elite graduate school and working at a prestigious firm at age 25, respectively (see Section 2 for more details). The sample consists of all students enrolled at Ivy-plus colleges in our college-specific sample for whom we have high school GPAs and other requisite variables to estimate the regressions.

Appendix Table 14: Differences in Post-College Outcomes by Applicant Characteristics at Ivy-Plus Colleges

| | No Controls | | Controlling for Test Score | | Controlling for Parent Income Bin | | Controlling for Test Score, Gender, and Race | | Controlling for All Observables | | Controlling for All Observables & Parent Income | |
|--|-------------|--------------------|----------------------------|--------------------|-----------------------------------|--------------------|--|--------------------|---------------------------------|--------------------|---|--------------------|
| | Difference | Controlling for VA | Difference | Controlling for VA | Difference | Controlling for VA | Difference | Controlling for VA | Difference | Controlling for VA | Difference | Controlling for VA |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| <i>Panel A: Predicted Top 1% Earnings Probability</i> | | | | | | | | | | | | |
| High Academic Rating vs. Low Academic Rating | 5.90 | 5.01 | 1.55 | 1.12 | 5.57 | 4.59 | 1.42 | 0.89 | 2.59 | 1.86 | 2.56 | 1.81 |
| | (0.26) | (0.26) | (0.37) | (0.36) | (0.27) | (0.27) | (0.37) | (0.36) | (0.43) | (0.43) | (0.43) | (0.43) |
| Athlete vs. Non-Athlete | -0.06 | -1.31 | 3.33 | 1.51 | -0.50 | -1.72 | 3.49 | 1.30 | 2.32 | 0.46 | 2.42 | 0.53 |
| | (0.32) | (0.32) | (0.33) | (0.33) | (0.32) | (0.32) | (0.34) | (0.34) | (0.39) | (0.39) | (0.39) | (0.39) |
| Legacy vs. Non-Legacy | -1.58 | -1.66 | -1.29 | -1.40 | -3.27 | -3.14 | -0.65 | -0.97 | -1.38 | -1.61 | -1.92 | -2.08 |
| | (0.26) | (0.26) | (0.26) | (0.26) | (0.26) | (0.26) | (0.26) | (0.26) | (0.29) | (0.29) | (0.29) | (0.29) |
| High Non-Academic Rating vs. Low Non-Academic Rating | 0.16 | -0.72 | 1.03 | 0.03 | 0.16 | -0.70 | 1.35 | 0.33 | 0.36 | -0.20 | 0.37 | -0.18 |
| | (0.25) | (0.25) | (0.25) | (0.25) | (0.25) | (0.25) | (0.25) | (0.25) | (0.35) | (0.35) | (0.35) | (0.35) |
| Second Quartile of SAT Distribution vs. First Quartile | 2.76 | 2.23 | 2.76 | 2.23 | 2.43 | 1.86 | 2.64 | 1.80 | 1.35 | 0.79 | 1.26 | 0.70 |
| | (0.18) | (0.18) | (0.18) | (0.18) | (0.19) | (0.19) | (0.19) | (0.19) | (0.22) | (0.22) | (0.22) | (0.22) |
| Third Quartile of SAT Distribution vs. First Quartile | 5.45 | 4.55 | 5.45 | 4.55 | 4.97 | 4.02 | 5.01 | 3.70 | 2.74 | 1.88 | 2.59 | 1.73 |
| | (0.20) | (0.20) | (0.20) | (0.20) | (0.21) | (0.21) | (0.21) | (0.21) | (0.26) | (0.26) | (0.26) | (0.26) |
| Fourth Quartile of SAT Distribution vs. First Quartile | 8.28 | 6.93 | 8.28 | 6.93 | 7.80 | 6.39 | 7.31 | 5.52 | 3.99 | 2.87 | 3.87 | 2.75 |
| | (0.21) | (0.21) | (0.21) | (0.21) | (0.22) | (0.22) | (0.23) | (0.22) | (0.29) | (0.29) | (0.29) | (0.29) |
| <i>Panel B: Elite Graduate School Attendance at Age 25</i> | | | | | | | | | | | | |
| High Academic Rating vs. Low Academic Rating | 7.48 | 7.31 | 4.30 | 4.23 | 6.96 | 6.75 | 4.57 | 4.48 | 4.46 | 4.34 | 4.40 | 4.27 |
| | (0.52) | (0.53) | (0.72) | (0.73) | (0.53) | (0.53) | (0.72) | (0.73) | (0.86) | (0.87) | (0.86) | (0.87) |
| Athlete vs. Non-Athlete | -8.35 | -8.72 | -3.92 | -4.37 | -8.76 | -9.13 | -2.41 | -2.92 | -2.13 | -2.50 | -2.18 | -2.57 |
| | (0.47) | (0.48) | (0.51) | (0.51) | (0.48) | (0.48) | (0.53) | (0.53) | (0.69) | (0.70) | (0.70) | (0.70) |
| Legacy vs. Non-Legacy | -2.26 | -2.42 | -1.90 | -2.06 | -3.56 | -3.70 | -1.16 | -1.36 | -0.91 | -1.13 | -1.42 | -1.63 |
| | (0.52) | (0.52) | (0.52) | (0.52) | (0.54) | (0.54) | (0.52) | (0.53) | (0.60) | (0.61) | (0.60) | (0.61) |
| High Non-Academic Rating vs. Low Non-Academic Rating | 0.14 | -0.05 | 1.09 | 0.87 | 0.25 | 0.07 | 0.97 | 0.75 | -0.50 | -0.60 | -0.47 | -0.56 |
| | (0.50) | (0.50) | (0.50) | (0.50) | (0.50) | (0.50) | (0.50) | (0.50) | (0.72) | (0.73) | (0.72) | (0.73) |
| Second Quartile of SAT Distribution vs. First Quartile | 3.42 | 3.43 | 3.42 | 3.43 | 2.94 | 2.94 | 4.41 | 4.37 | 2.41 | 2.37 | 2.23 | 2.20 |
| | (0.40) | (0.40) | (0.40) | (0.40) | (0.41) | (0.41) | (0.41) | (0.42) | (0.47) | (0.47) | (0.47) | (0.48) |
| Third Quartile of SAT Distribution vs. First Quartile | 6.47 | 6.40 | 6.47 | 6.40 | 5.81 | 5.72 | 7.83 | 7.68 | 4.40 | 4.28 | 4.14 | 4.01 |
| | (0.42) | (0.42) | (0.42) | (0.42) | (0.43) | (0.44) | (0.44) | (0.45) | (0.54) | (0.54) | (0.54) | (0.54) |
| Fourth Quartile of SAT Distribution vs. First Quartile | 10.40 | 10.16 | 10.40 | 10.16 | 9.62 | 9.36 | 11.85 | 11.52 | 7.11 | 6.84 | 6.84 | 6.56 |
| | (0.44) | (0.45) | (0.44) | (0.45) | (0.46) | (0.46) | (0.47) | (0.48) | (0.61) | (0.62) | (0.61) | (0.62) |
| <i>Panel C: Employment at Prestigious Firm at Age 25</i> | | | | | | | | | | | | |
| High Academic Rating vs. Low Academic Rating | 10.56 | 8.95 | 5.05 | 4.19 | 9.91 | 7.98 | 5.26 | 4.15 | 5.83 | 4.29 | 5.88 | 4.24 |
| | (0.86) | (0.85) | (1.20) | (1.18) | (0.88) | (0.87) | (1.20) | (1.19) | (1.50) | (1.50) | (1.51) | (1.51) |
| Athlete vs. Non-Athlete | -3.93 | -9.62 | 1.85 | -4.83 | -4.64 | -10.28 | 1.34 | -6.04 | 0.96 | -4.75 | 1.05 | -4.77 |
| | (0.94) | (0.95) | (0.98) | (0.98) | (0.95) | (0.95) | (1.00) | (1.01) | (1.20) | (1.20) | (1.20) | (1.20) |
| Legacy vs. Non-Legacy | 0.50 | -1.43 | 0.95 | -1.05 | -2.05 | -3.62 | 0.45 | -1.91 | -2.69 | -4.35 | -3.10 | -4.68 |
| | (0.84) | (0.82) | (0.84) | (0.82) | (0.86) | (0.84) | (0.84) | (0.83) | (0.94) | (0.94) | (0.94) | (0.94) |
| High Non-Academic Rating vs. Low Non-Academic Rating | 4.15 | 1.08 | 5.70 | 2.35 | 4.12 | 1.15 | 5.41 | 2.12 | 2.13 | 0.29 | 2.12 | 0.31 |
| | (0.75) | (0.74) | (0.75) | (0.74) | (0.75) | (0.74) | (0.74) | (0.75) | (1.15) | (1.14) | (1.15) | (1.14) |
| Second Quartile of SAT Distribution vs. First Quartile | 4.97 | 4.27 | 4.97 | 4.27 | 4.41 | 3.52 | 4.86 | 3.49 | 2.01 | 1.25 | 1.96 | 1.15 |
| | (0.64) | (0.64) | (0.64) | (0.64) | (0.66) | (0.65) | (0.67) | (0.67) | (0.77) | (0.77) | (0.77) | (0.77) |
| Third Quartile of SAT Distribution vs. First Quartile | 8.90 | 7.45 | 8.90 | 7.45 | 8.12 | 6.44 | 8.81 | 6.44 | 3.80 | 2.52 | 3.74 | 2.40 |
| | (0.66) | (0.65) | (0.66) | (0.65) | (0.68) | (0.67) | (0.71) | (0.70) | (0.86) | (0.86) | (0.87) | (0.86) |
| Fourth Quartile of SAT Distribution vs. First Quartile | 13.65 | 11.25 | 13.65 | 11.25 | 12.87 | 10.17 | 13.76 | 10.31 | 6.55 | 4.43 | 6.52 | 4.31 |
| | (0.67) | (0.66) | (0.67) | (0.66) | (0.70) | (0.69) | (0.74) | (0.73) | (0.97) | (0.97) | (0.97) | (0.97) |
| <i>Panel D: Employment at Elite Firm at Age 25</i> | | | | | | | | | | | | |
| High Academic Rating vs. Low Academic Rating | 7.67 | 6.30 | 3.93 | 3.31 | 7.15 | 5.50 | 4.02 | 3.20 | 3.92 | 2.63 | 3.95 | 2.58 |
| | (0.86) | (0.85) | (1.20) | (1.18) | (0.88) | (0.87) | (1.20) | (1.19) | (1.49) | (1.49) | (1.49) | (1.50) |
| Athlete vs. Non-Athlete | -5.11 | -10.30 | -0.71 | -6.73 | -5.56 | -10.70 | -1.41 | -7.99 | -1.13 | -6.24 | -1.10 | -6.30 |
| | (0.91) | (0.92) | (0.95) | (0.96) | (0.91) | (0.92) | (0.97) | (0.98) | (1.19) | (1.19) | (1.19) | (1.19) |
| Legacy vs. Non-Legacy | 0.92 | -0.96 | 1.29 | -0.67 | -0.57 | -2.13 | 0.60 | -1.63 | -2.02 | -3.48 | -2.05 | -3.45 |
| | (0.83) | (0.82) | (0.83) | (0.82) | (0.85) | (0.84) | (0.84) | (0.83) | (0.94) | (0.94) | (0.95) | (0.94) |
| High Non-Academic Rating vs. Low Non-Academic Rating | 2.78 | 0.27 | 3.93 | 1.18 | 2.79 | 0.38 | 3.60 | 0.91 | 1.19 | -0.19 | 1.17 | -0.18 |
| | (0.74) | (0.73) | (0.74) | (0.74) | (0.74) | (0.73) | (0.75) | (0.74) | (1.13) | (1.13) | (1.13) | (1.13) |
| Second Quartile of SAT Distribution vs. First Quartile | 3.59 | 3.02 | 3.59 | 3.02 | 3.23 | 2.50 | 3.28 | 2.16 | 1.38 | 0.74 | 1.40 | 0.71 |
| | (0.65) | (0.64) | (0.65) | (0.64) | (0.66) | (0.65) | (0.67) | (0.67) | (0.77) | (0.78) | (0.78) | (0.78) |
| Third Quartile of SAT Distribution vs. First Quartile | 6.56 | 5.33 | 6.56 | 5.33 | 6.08 | 4.65 | 6.25 | 4.25 | 2.75 | 1.65 | 2.79 | 1.63 |
| | (0.66) | (0.65) | (0.66) | (0.65) | (0.68) | (0.67) | (0.71) | (0.70) | (0.87) | (0.87) | (0.87) | (0.87) |
| Fourth Quartile of SAT Distribution vs. First Quartile | 10.29 | 8.21 | 10.29 | 8.21 | 9.80 | 7.44 | 10.24 | 7.27 | 5.01 | 3.11 | 5.04 | 3.05 |
| | (0.67) | (0.67) | (0.67) | (0.67) | (0.70) | (0.69) | (0.74) | (0.73) | (0.97) | (0.97) | (0.97) | (0.97) |

Notes: This table replicates estimates from Figure 14 using other student characteristics and additional controls. In each pair of columns, the first (odd-numbered) column presents "raw" differences in outcomes between the two groups listed in the relevant row, controlling for certain variables but without adjusting for differences in the observational value-added of the colleges' applicants attended (as in the left bar in each pair in Figure 14a). The second (even-numbered) column presents the same estimate, subtracting out the difference in the observational value-added of college attended multiplied by the ratio of the waitlist-design treatment effect estimate to the observational VA estimate reported in Columns 1 and 5 of Appendix Table 8 (as in Figures 14b-d). Each row reports an estimate from a separate regression. Each pair of columns includes a different set of controls when estimating both the raw effects and differences in observational VA: in Columns 1-2, no additional controls; Columns 3-4, controls for a quintic in test scores as well as indicators for gender and race/ethnicity; Columns 5-6, controls for gender and race/ethnicity, indicators of the combination of student's academic and non-academic ratings, the year applied, an indicator for early applications, first-generation status, a quadratic of high school GPA, and teacher, guidance counselor, and alumni ratings; and Columns 11-12, controls for all variables used in 9-10 as well as the parent income bins. The four panels each consider different outcomes; see Section 2 for definitions of these outcomes. The sample consists of students either admitted or offered a place on the waitlist at the Ivy-plus college for which we have internal data in our college-specific sample. The first row and fourth rows of each panel, which compare academic and non-academic ratings, limit the sample to students admitted or offered a place on the waitlist at the Ivy-Plus college with the most granular rating data. Baseline rates of outcomes for low academic rating, non-athlete, non-legacy, and low non-academic rating students with SATs in the first quartile (below 1400) are 10.4% (Panel A), 6.8% (Panel B), 17.4% (Panel C), and 19.2% (Panel D).

Appendix Table 15: Predicting Top 1% Income at Age 33 Using Age 22-25 Employment Information

| | Age 25 Firm (1) | Ages 22-25 Firm (2) | Ages 22-25 Income (3) | Ages 22-25 Firm and Income (4) |
|--|--------------------|------------------------|--------------------------|-----------------------------------|
| R-Squared of Prediction | 0.11 | 0.16 | 0.02 | 0.16 |
| Waitlist Treatment Effect on Predicted Top 1% | 2.13 (0.60) | 2.72 (0.71) | -0.40 (0.14) | 2.68 (0.71) |
| Waitlist Reject Mean | 13.76 | 15.94 | 10.21 | 16.02 |

Notes: This table presents statistics from four different prediction models using early-career information to predict top 1% income at age 33. In each column, we estimate a different model using data from earlier cohorts (born 1977-1988) who attend colleges in Barron's tiers 1-4 (as defined in Chetty et al. 2020). We then present statistics from that model: the in-sample R-squared of the top 1% prediction on actual top 1% outcomes (Row 1), the treatment effect obtained from the waitlist estimate (using the "Raw Means" specification from Figure 8b) on the predicted outcome from that model; and the mean of the prediction for students rejected from the waitlist. In Column 1, we predict top 1% income at age 33 with fixed effects for the firm at which a student was employed (or the graduate school they attend) at age 25 only. In Column 2, we add additional fixed effects for the firm (or graduate school) at ages 22-24. In Column 3, we instead predict using incomes interacted with an indicator for graduate school attendance at each age 22-25 (each entering linearly in the prediction regression). In Column 4, we combine the models in Columns 2 and 3 to predict with income and firm (or graduate school) at each age. See Appendix D for more details.

Appendix Table 16: Variance Decomposition of Predicted Top 1% Income at Age 33 by Industry at Age 25

| | Share of all Observations (1) | Variance (2) | Variance as Share of Total Variance (3) |
|--|----------------------------------|-----------------|--|
| <i>Panel A: Finance, Non-Finance</i> | | | |
| Finance (52) | 0.069 | 0.054 | 0.150 |
| Non-Finance | 0.738 | 0.021 | 0.632 |
| Missing NAICS Code | 0.193 | 0.006 | 0.050 |
| Between Groups | | 0.004 | 0.167 |
| Overall | 1.000 | 0.025 | 1.000 |
| <i>Panel B: Finance/Consulting/Tech, Non-Finance/Consulting/Tech</i> | | | |
| Finance, Consulting, or Tech (51, 52, 54) | 0.330 | 0.043 | 0.568 |
| Other NAICS Code | 0.477 | 0.010 | 0.201 |
| Missing NAICS Code | 0.193 | 0.006 | 0.050 |
| Between Groups | | 0.004 | 0.181 |
| Overall | 1.000 | 0.025 | 1.000 |
| <i>Panel C: Finance, Consulting, Tech</i> | | | |
| Consulting (54) | 0.196 | 0.024 | 0.193 |
| Finance (52) | 0.069 | 0.054 | 0.150 |
| Tech (51) | 0.065 | 0.059 | 0.153 |
| Other NAICS Code | 0.477 | 0.010 | 0.201 |
| Missing NAICS Code | 0.193 | 0.006 | 0.050 |
| Between Groups | | 0.006 | 0.252 |
| Overall | 1.000 | 0.025 | 1.000 |
| <i>Panel D: Finance/Consulting/Tech, Non-Profit/Public</i> | | | |
| Finance, Consulting, or Tech (51, 52, 54) | 0.330 | 0.043 | 0.568 |
| Non-Profit or Public (61, 62, 92) | 0.232 | 0.007 | 0.065 |
| Other NAICS Code | 0.246 | 0.014 | 0.136 |
| Missing NAICS Code | 0.193 | 0.006 | 0.050 |
| Between Groups | | 0.004 | 0.181 |
| Overall | 1.000 | 0.025 | 1.000 |

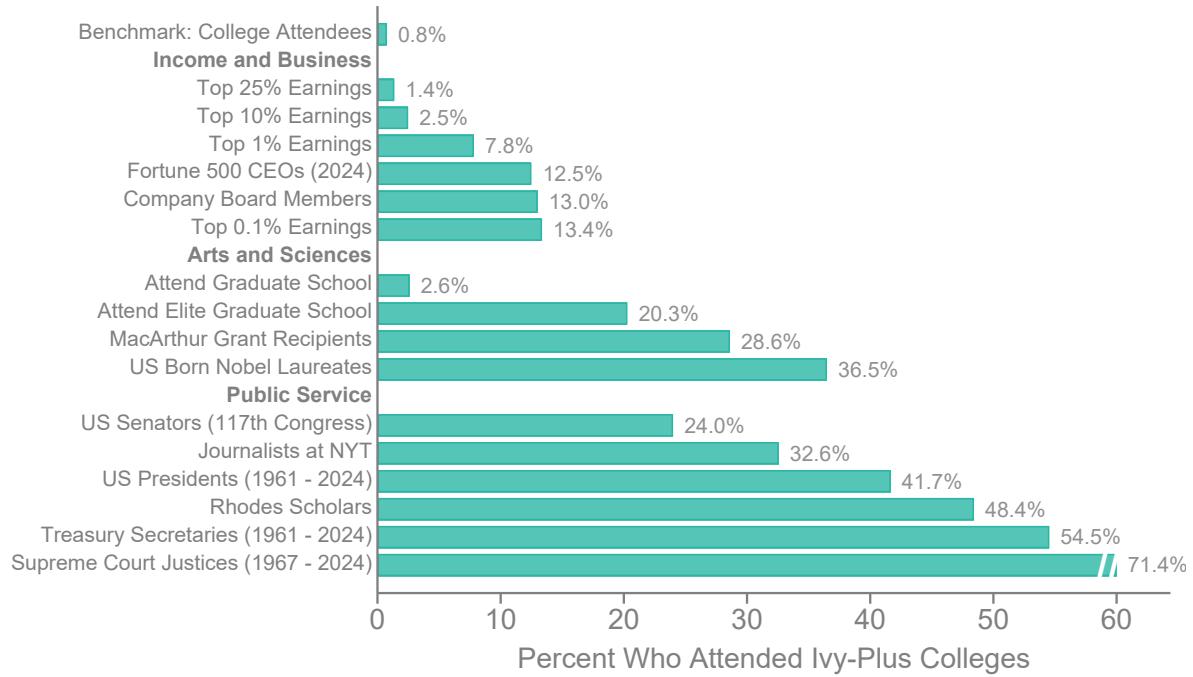
Notes: This table presents the results of four separate variance decompositions of our predicted top 1% variable by NAICS code of age 25 firm in the sample of admitted or waitlisted students in our Ivy-Plus admissions sample. NAICS codes are available for 80.7% of firms at which students in our sample are employed. We classify these firms into key industries based on their two-digit NAICS codes (Technology 51, Finance 52, Consulting 54, Non-Profit 61 or 62, and Public Sector 92). In each panel, we provide the decomposition across a different grouping of industries. In Column 1, we report the share of individuals in our sample who fall into each grouping. In Column 2, we report the variance of the predicted top 1% measure within each group, along with the variance of our predicted top 1% measure between categories, and the overall variance of the predicted top 1% measure across individuals in the sample. In Column 3, we express the variance components in Column 2 as a fraction of the total variance by dividing the product of the share in Column 1 and the variance in Column 2 by the overall variance.

Appendix Table 17: Examples of High Schools by High School Type, by Metro Area

| Area | Public | | Private | |
|-------------------|------------------------------------|------------------------------|-----------------------------------|----------------------------|
| | <i>Disadvantaged</i> | <i>Advantaged</i> | <i>Religious</i> | <i>Non-Religious</i> |
| New York | Forest Hills High School | Scarsdale High School | Fordham Preparatory School | Horace Mann School |
| Boston | Somerville High School | Newton South High School | Boston College High School | Milton Academy |
| Atlanta | Midtown High School | Northview High School | Westminster School | Pace Academy |
| Chicago | Crete-Monee High School | New Trier High School | St. Ignatius College Prep | Lake Forest Academy |
| Los Angeles | Pasadena High School | Palos Verdes High School | Loyola High School | Harvard-Westlake School |
| San Francisco Bay | George Washington High School | Palo Alto High School | Archbishop Mitty High School | Harker School |
| Houston | Jersey Village High School | Clements High School | Strake Jesuit College Preparatory | Kinkaid School |
| Miami | Southwest Miami Senior High School | Cypress Bay High School | Belen Jesuit Preparatory School | Pine Crest School |
| Phoenix | Mountain View High School | Desert Vista High School | Northwest Christian Academy | Brophy College Preparatory |
| Philadelphia | Coatesville High School | North Penn High School | Archbishop Ryan High School | Germantown Friends School |
| Washington DC | Springbrook High School | Thomas Jefferson High School | National Cathedral School | Sidwell Friends School |
| Dallas-Ft Worth | Rowlett High School | Carroll High School | St. Marks School of Texas | The Hockaday School |

Notes: This table presents illustrative examples of high schools from each of the four categories in Appendix Figure 16 in twelve large metropolitan areas in the U.S. We break public high schools into two groups based on their percentile on high school challenge indicators that capture educational opportunities or disadvantages in the high school environment, variables that feed into the CollegeBoard Landscape tool. We classify high schools that fall in the top 20% of this index of advantage as “advantaged.” The schools in these examples were identified using publicly available data; they were not chosen based on their presence in any of our confidential datasets or based on their estimated fixed effects.

Figure 1: Share of Individuals in Leadership Positions who Attended Ivy-Plus Colleges



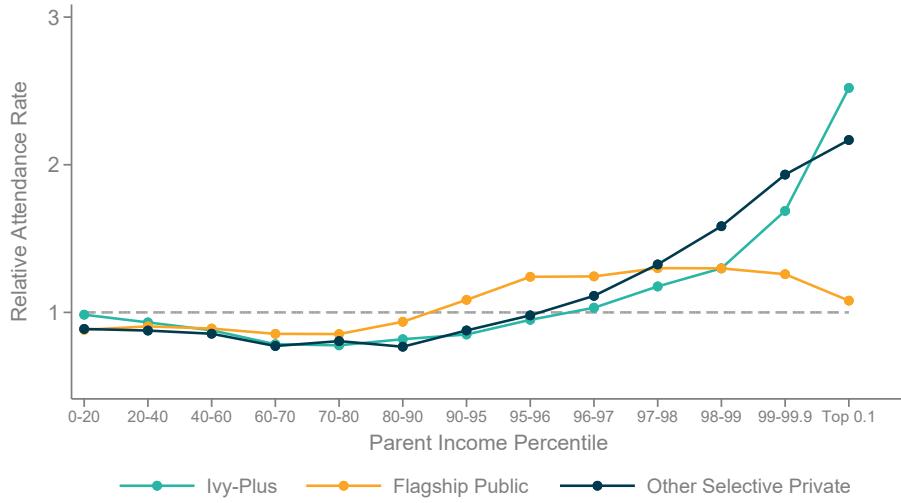
Notes: Figure 1 shows the proportion of individuals in various subgroups who attended an Ivy-Plus college (the eight Ivy-League colleges, Chicago, Duke, Stanford, and MIT) as an undergraduate. See Appendix Figure 1 for comparable statistics for other private colleges and flagship public colleges. For definitions and sources for each of these outcome variables, see Appendix A.

Figure 2: Attendance Rates at Selective Colleges by Parental Income

(a) Ivy-Plus Attendance Rates for Students Scoring at 99th Percentile of SAT/ACT, by Parental Income

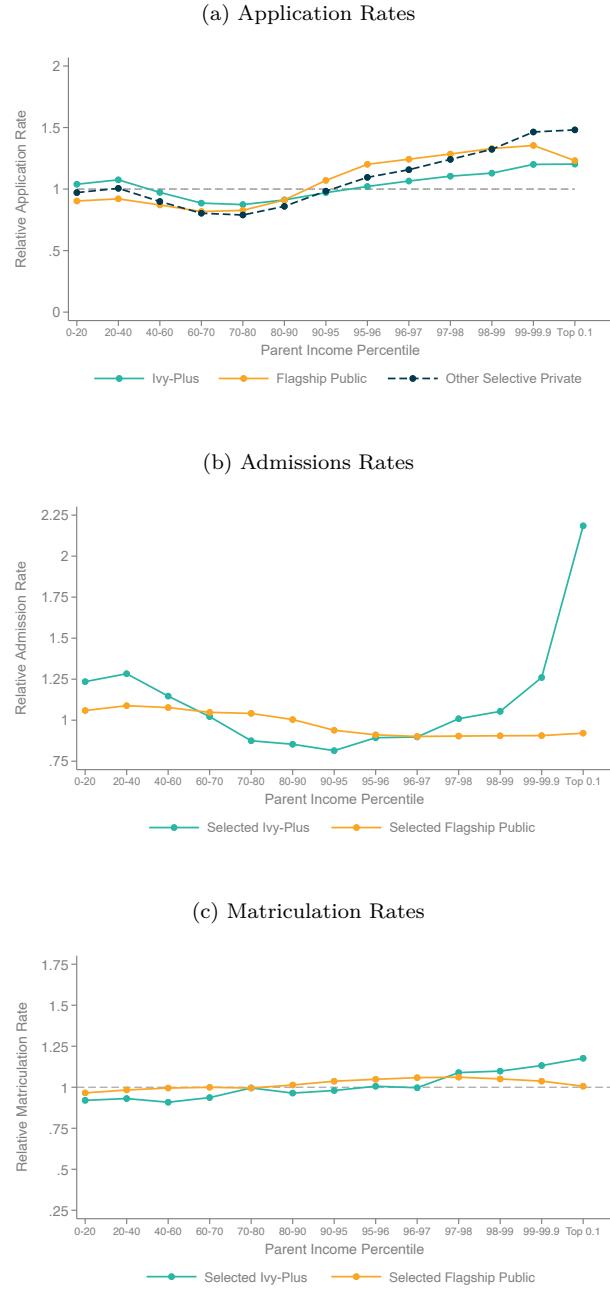


(b) Attendance Rates at Selective Colleges by Parental Income, Controlling for Test Scores



Notes: Figure 2a plots the share of individuals with an SAT score of 1510 (out of 1600) or an ACT composite score of 34 (out of 36) who attend an Ivy-Plus college, by parental income. To construct the series for Ivy-Plus colleges in Figure 2b, we calculate the attendance rate at each Ivy-Plus college (separately) for students in each parent income bin and at each test score level. For each college and within each parent-income bin, we then average together the attendance rates from different test score levels, where the weight on each test score level is the fraction of attending students at that specific college with that specific test score. This procedure reweights the distribution of test scores at each parent income level to match the overall distribution of test scores for students attending each college. We then calculate the relative attendance rate at each college by dividing the resulting test-score-reweighted attendance rate by the mean test-score-reweighted average attendance rate (across students from all parent-income bins). Finally, we take an unweighted average of the 12 college-specific series. Figure 2b plots this relative attendance rate series for the twelve Ivy-Plus colleges, as well as similarly constructed relative attendance rates for the 12 other highly selective private colleges and 9 flagship public colleges listed in Appendix Table 1. The sample for both panels is our pipeline analysis sample: the set of students who were on pace to graduate from high school and took either the SAT or the ACT in 2011, 2013, or 2015 and whom we can link to parent incomes in the tax data. See Section 2 for details on sample construction and variable definitions.

Figure 3: Pipeline Decomposition of Attendance Rates at Selective Colleges by Parental Income, Controlling for Test Scores



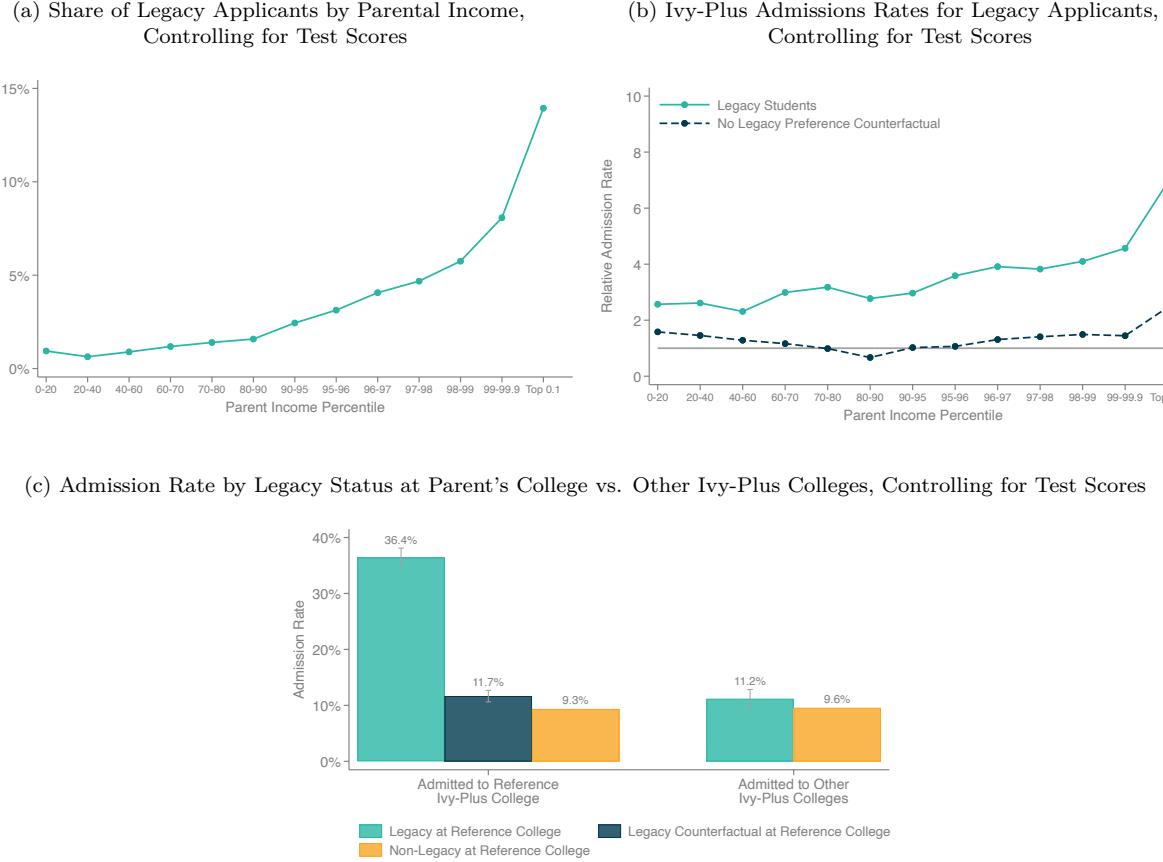
Notes: Figure 3a replicates Figure 2b but with application rates rather than attendance rates, where application rates are predicted using score sending data as described in Appendix C. Figure 3b and Figure 3c plot admissions and matriculation (or yield) rates by parental income, controlling for test scores. We reweight students within each parent income bin on test scores using the same method as in Figure 2; see notes to Figure 2 for details. The sample for Figures 3b and 3c is our college-specific sample, a selected subset of Ivy-Plus and flagship public colleges for which we have linked internal admissions data.

Figure 4: Share of Admitted Students who are Recruited Athletes, by Parental Income



Notes: Figure 4a and Figure 4b plot the fraction of admits who are recruited athletes by parent income bin at Ivy-Plus and flagship public schools, respectively. The dashed lines show the mean share of admits who are recruited athletes. The sample for these figures is our college-specific sample, a selected subset of Ivy-Plus and flagship public schools for which we have linked internal admissions data.

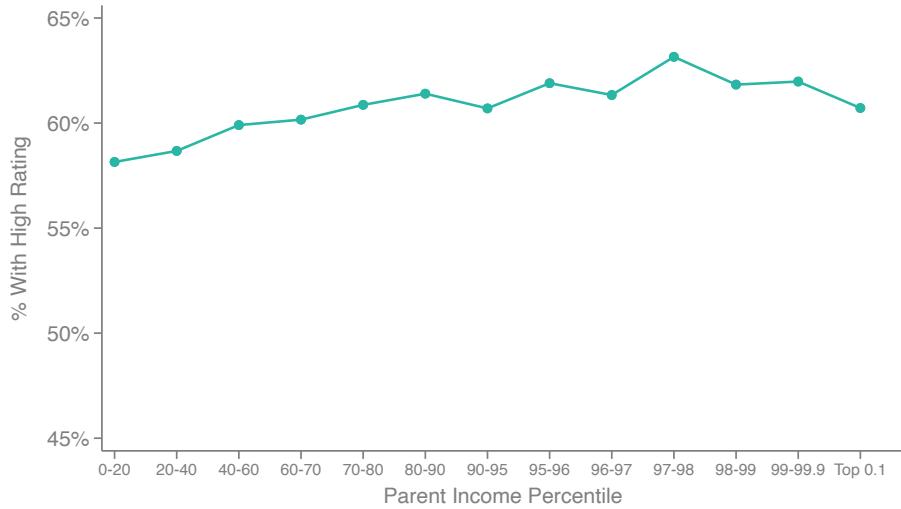
Figure 5: Ivy-Plus Legacy Applicant Shares and Admissions Rates, by Parental Income



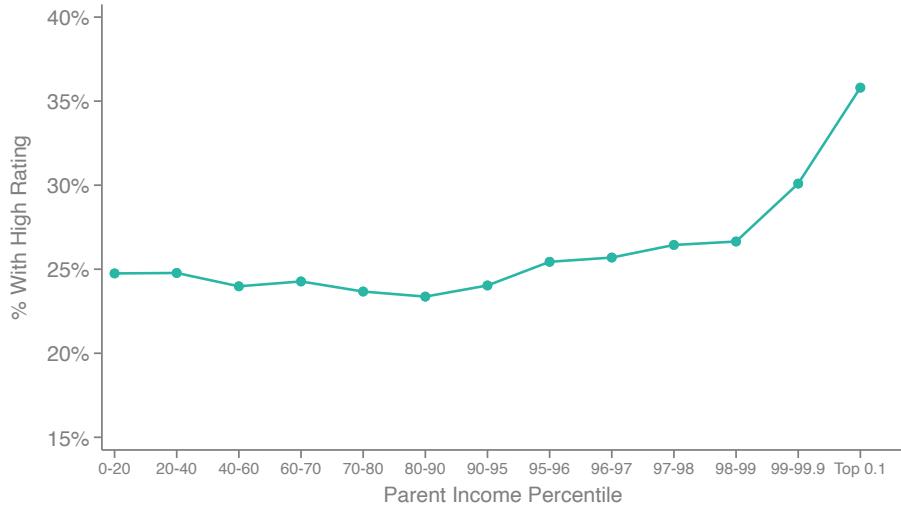
Notes: Figure 5a plots the share of non-athlete applicants to selected Ivy-Plus colleges who are children of alumni (i.e., legacy students) by parent income level, controlling for test scores using the same reweighting procedure as in Figure 2b. Figure 5b plots two series. The solid (green) series plots admissions rates for legacy applicants in each parent income bin, reweighting those applicants across test score bins to match the distribution of test score for all attendees (as in Figure 2b). The dashed (dark blue) series replicates the solid series using a counterfactual admissions rate for legacy students if they did not benefit from legacy preferences in admissions but had otherwise identical application credentials. We divide the admissions rates in both series by the mean test-score-reweighted predicted counterfactual admissions rate for all applicants, so that the values can be interpreted as admissions rates relative to the average applicant absent legacy preferences. To calculate counterfactual admissions rates absent legacy preferences, we first estimate a linear probability model to predict admissions of non-legacy students using indicators for race, gender, first-generation status, entering cohort, and application round, fixed effects for the full tuple of admissions office ratings, high-school GPA (where available), parent income bin, and high-school fixed effects, reweighting students to match all attendees on test score. We then apply the coefficients from this admissions model for non-legacies to predict a counterfactual admissions rate based on the individual characteristics of each legacy student. Figure 5c compares admissions rates for legacy vs. non-legacy students across colleges for non-athletes who apply regular decision to multiple Ivy-Plus colleges in our college-specific sample. The first bar plots mean admissions rates at Ivy-Plus colleges for applicants whose parents are alumni of that college. The second bar plots the mean counterfactual non-legacy admissions rate (constructed as above) for the same group of students. To construct the remaining bars, we regress admissions rates at each Ivy-Plus college on indicators for legacy status at that college, legacy status at other Ivy-Plus colleges, and a quintic in SAT/ACT scores. The third bar plots the implied admissions rate for non-legacy applicants (controlling for SAT scores) based on this regression. The fourth bar plots the admissions rate at other Ivy-Plus colleges (i.e., a college the applicant's parents did not attend) for legacy students at a given college. The fifth bar plots the admissions rate for non-legacy applicants at other Ivy-Plus colleges. All results are based on the selected set of Ivy-Plus colleges for which we have linked internal admissions data; see Section 2 for details.

Figure 6: Admissions Office Ratings of Applicants by Parental Income, Controlling for Test Score

(a) Academic Ratings by Parental Income, Controlling for Test Scores

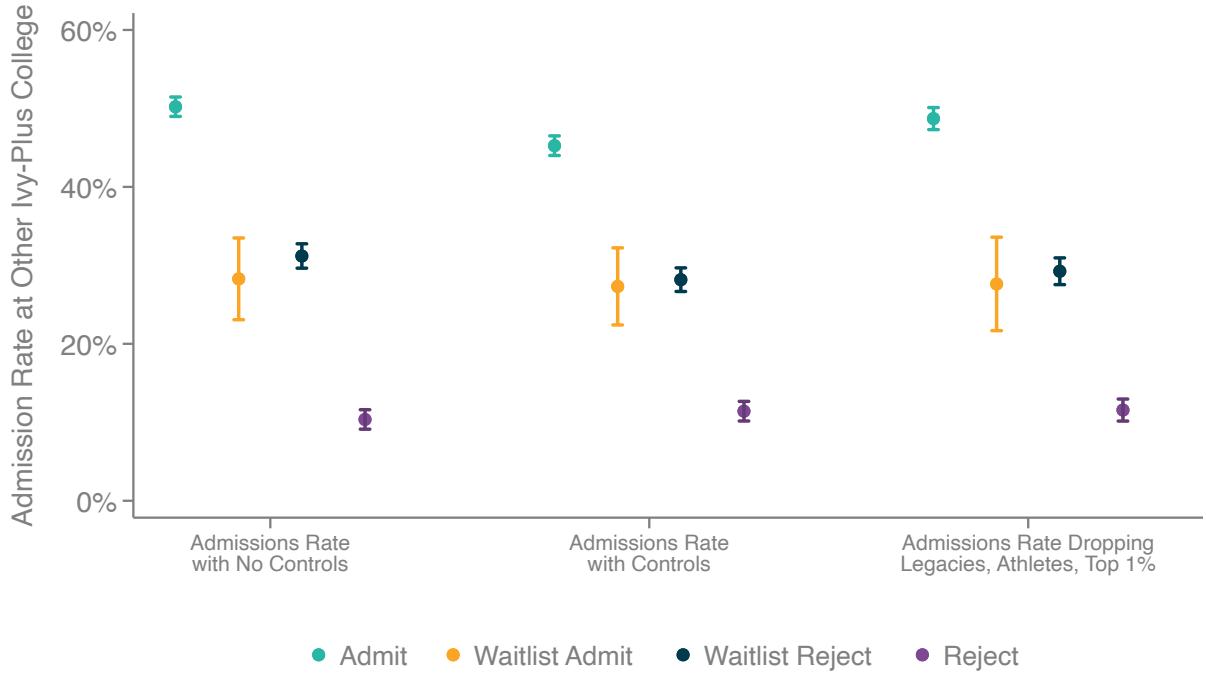


(b) Non-Academic Ratings by Parental Income, Controlling for Test Scores



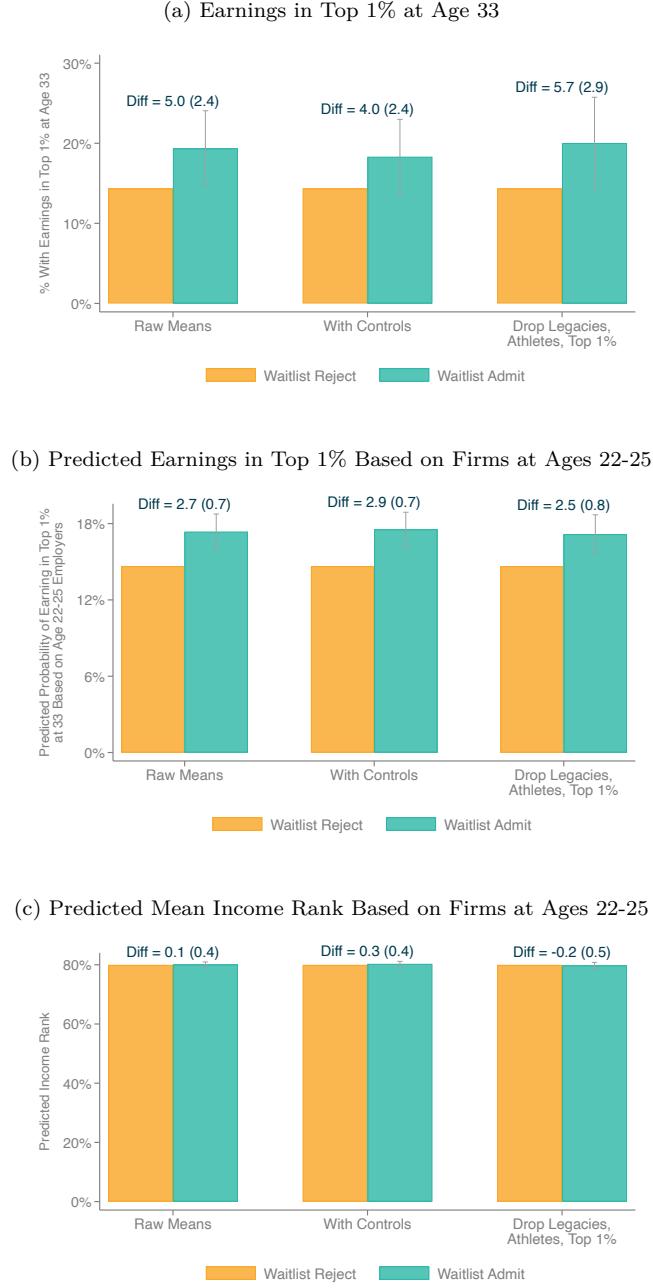
Notes: Figure 6 plots the proportion of Ivy-Plus applicants receiving high admissions ratings on various dimensions by parent-income bin, reweighting applicants to control for test scores as in Figure 2. Figure 6a considers academic ratings, and Figure 6b considers non-academic ratings. See Appendix Figure 14 for analogous figures that pool all Ivy-Plus colleges in our sample and use coarser ratings.

Figure 7: Multiple-Rater Test for Idiosyncratic Variation in Admissions Decisions



Notes: Figure 7 tests whether admissions decisions are driven by idiosyncratic variation by examining the relationship between admissions decisions at a given Ivy-Plus college and other Ivy-Plus colleges. Each block of four dots plots admissions rates at a lower-ranked Ivy-Plus college (based on revealed preference) by the admissions outcome at another higher-ranked Ivy-Plus college (admitted directly, admitted off the waitlist, rejected off the waitlist, and rejected without being waitlisted). The first block includes no additional controls. The second block of four dots repeats the first block but controls for a quintic function of test scores and includes fixed effects for parent-income bin, race, gender, recruited athlete status, legacy status, and home state. The third block of four dots repeats the second block, but dropping all students who are legacies, recruited athletes, or with parental incomes in the top 1%. The intervals are 95% confidence intervals. All estimates are based on individuals who applied to at least two Ivy-Plus colleges in our college-specific sample.

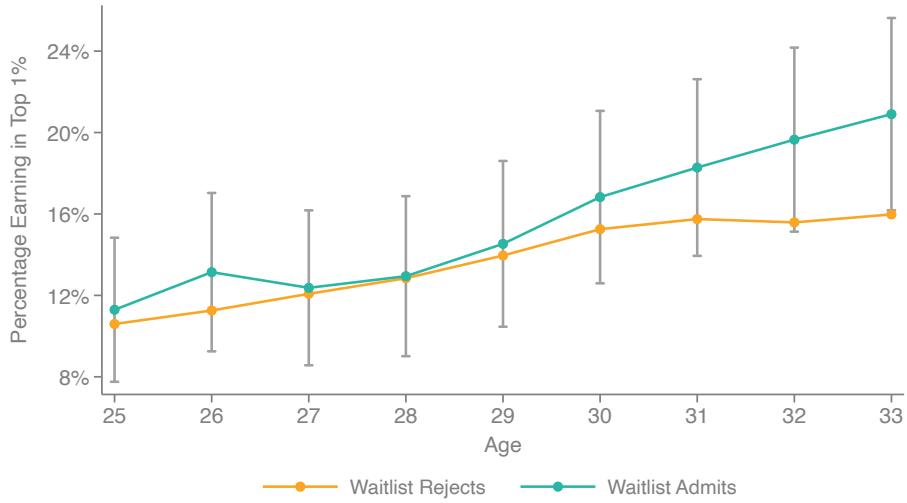
Figure 8: Treatment Effects of Ivy-Plus Admissions on Income for Waitlisted Applicants



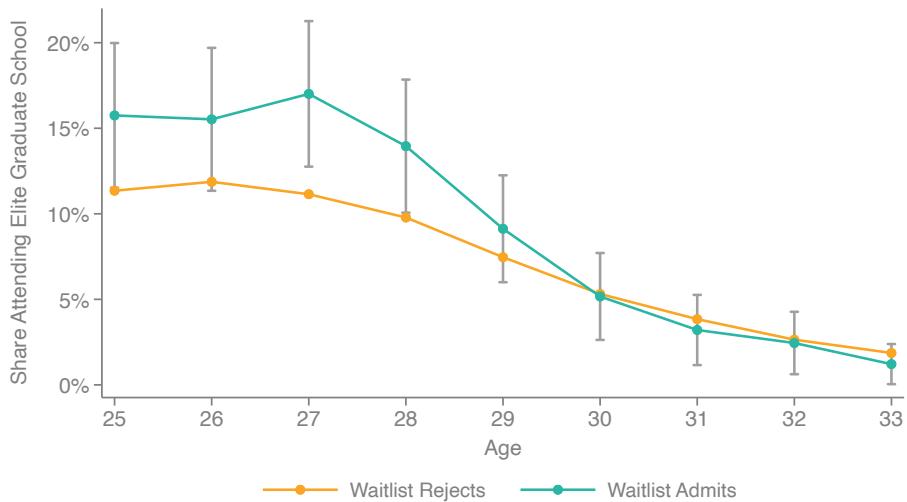
Notes: Figure 8a shows the treatment effect of Ivy-Plus admission on the probability of reaching the top 1% of the income distribution at age 33 by plotting outcomes for students admitted vs. rejected from the waitlist at an Ivy-Plus college. Income is individual income, defined as household income from the 1040 return minus spousal wage and self-employment earnings (if married). Orange bars (left side in each pair) plot the average outcome for waitlist rejects; green bars (right side in each pair) plot the orange bar plus the estimated treatment-on-the-treated (TOT) effect. We estimate the TOT of attending an Ivy-Plus college in the sample of waitlisted applicants by regressing an indicator for reaching the top 1% on matriculation, instrumenting for matriculation using an indicator for admission, with varying control vectors. In the first pair of bars, we estimate the TOT using only fixed effects for the college at which the student was waitlisted as controls. The second pair of bars further includes controls for a quintic in test scores, indicators for parent income bins, gender, race, state, recruited athlete, and legacy status. The third pair of bars replicates second pair of bars, except excluding legacies, athletes, and applicants with parental income in the top 1%. Figure 8b replicates Figure 8a using the predicted top 1% share based on the firms at which the individual works at ages 22-25 (see Section 2 for details); Figure 8c uses mean predicted income rank based on firms at ages 22-25 instead. Standard errors are clustered by individual, with whiskers denoting 95% confidence intervals for the TOT estimates. All estimates are based on the set of Ivy-Plus colleges in our college-specific analysis sample.

Figure 9: Treatment Effects of Ivy-Plus Admissions, by Age

(a) Share in Top 1%

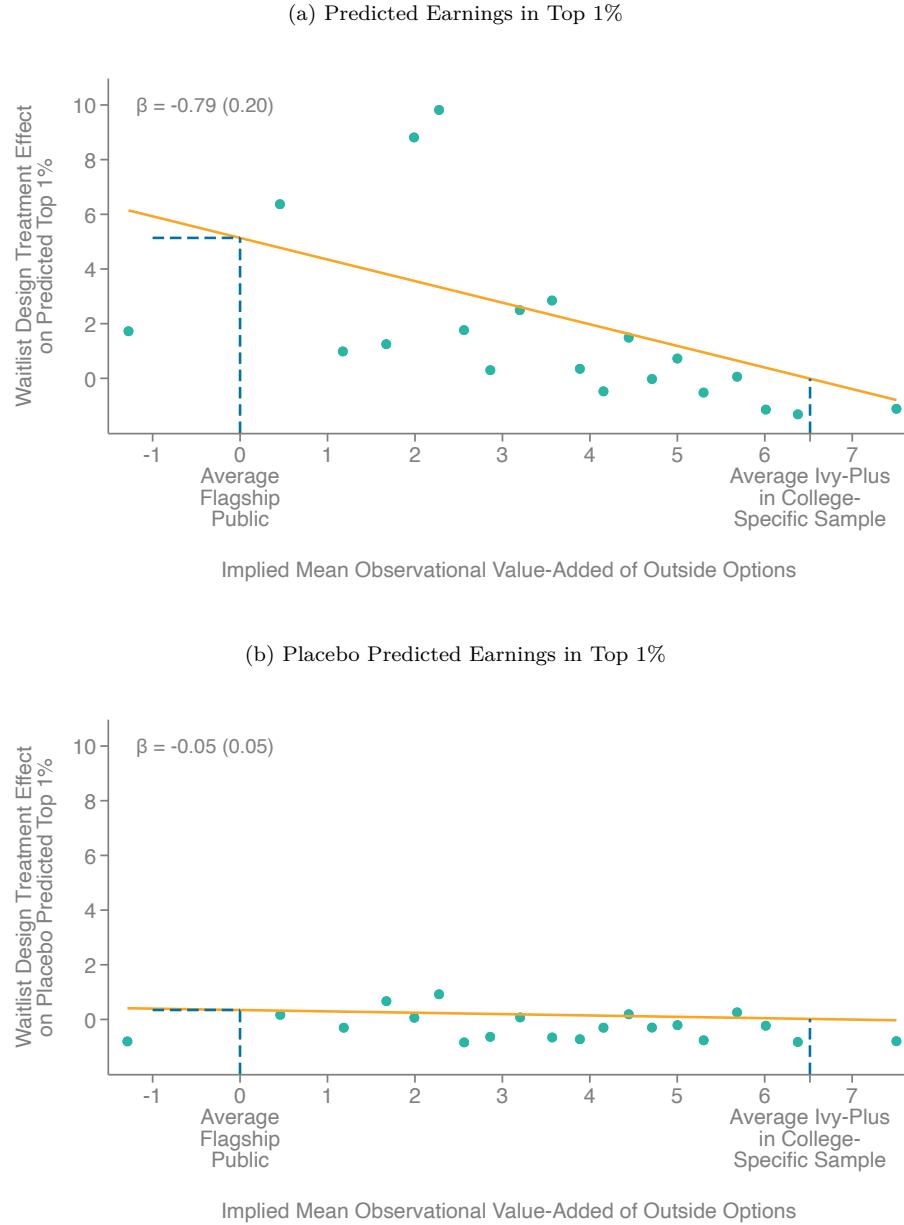


(b) Elite Graduate School Attendance



Notes: Figure 9a shows the estimated treatment-on-the-treated effect of Ivy-Plus admission for waitlisted applicants on the probability of having individual income in the top 1% of the age-specific income distribution at various ages, from 25 to 33. The orange line plots the average outcome for waitlist rejects; green bars plot the orange bar plus the estimated treatment effect at each age. The treatment effect is calculated using separate two-stage-least-squares regressions for each age using the same specification as in the leftmost pair of bars in Figure 8a; the estimates at age 33 in this figure replicate the estimate from the left pair of bars in Figure 8a. Figure 9b replicates Figure 9a using an indicator variable for attending an elite graduate school at each age as the outcome variable. All estimates are based on the set of Ivy-Plus colleges in our college-specific analysis sample and are based on a balanced panel of individuals observed until age 33.

Figure 10: Heterogeneity in Treatment Effects by Strength of Outside Options



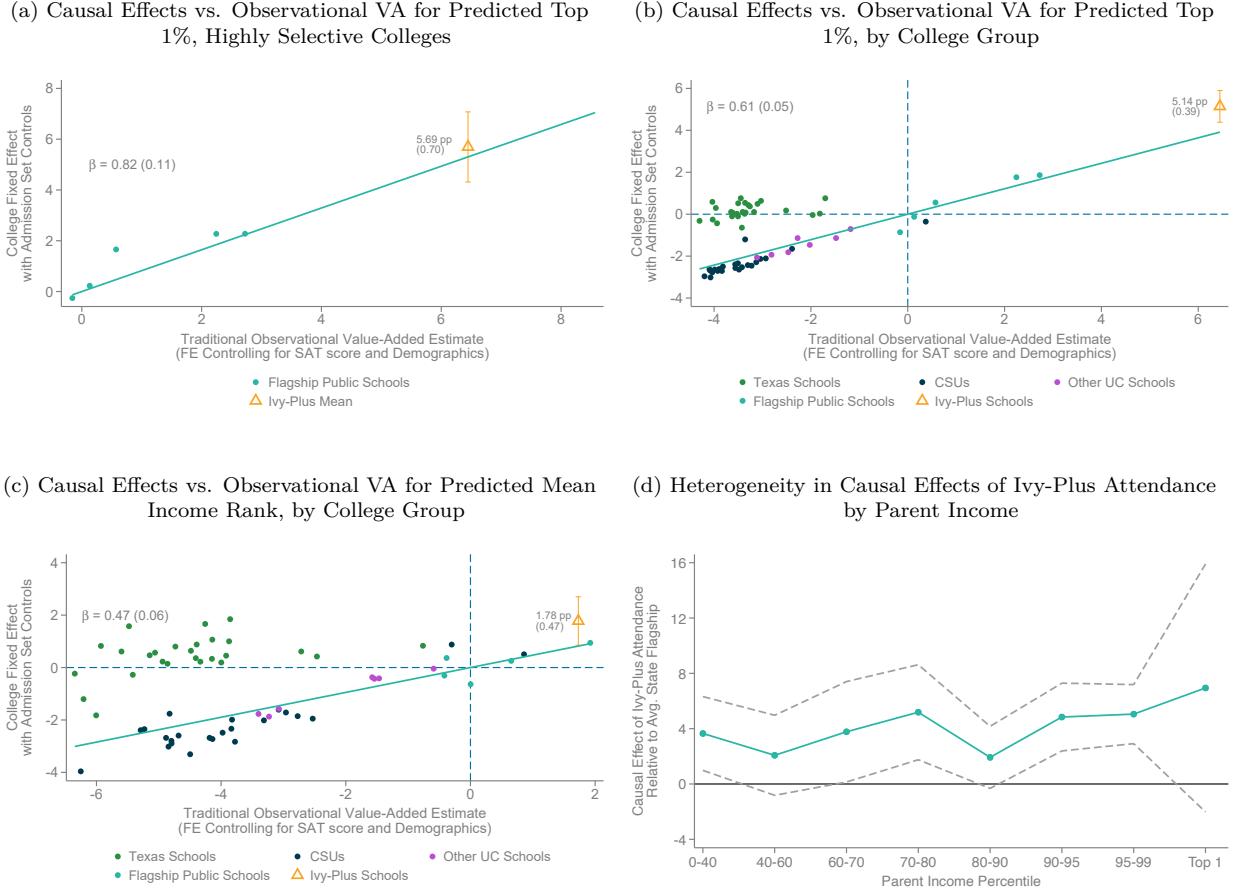
Notes: Figure 10 shows how the treatment effect of Ivy-Plus admission from the waitlist varies with the strength of an applicant's outside options. We place Ivy-Plus applicants into subgroups j based on their home state, parent income, race, and the Ivy-Plus college to which they applied. Within each group j , we measure the strength of students' outside options based on the average observational value-added of the colleges that students who are rejected from the waitlist attend (see Appendix J for details). We then divide students into 20 bins based on this strength of outside options variable. The x-coordinate of each of the 20 points is the mean implied observational VA of outside options within each bin. To construct the y coordinates, we regress the predicted top 1% outcome on indicators for Ivy-Plus admission interacted with the 20 outside option strength dummies and indicators for the Ivy-Plus college to which they applied, using the sample of waitlisted Ivy-Plus applicants as in Figure 8b. We then divide these coefficients by the "first stage" effect of the strength of outside options on actual college VA (see Appendix J). We also report the 2SLS regression slope (and the implied best fit line) corresponding to the plotted points, estimated using the specification in Column 1 of Appendix Table 8. Figure 10b replicates Figure 10a using placebo predicted top 1% outcomes based on a set of predetermined application characteristics (listed in Appendix J) as the y variable. All estimates are based on the set of Ivy-Plus colleges in our college-specific analysis sample.

Figure 11: Treatment Effects of Ivy-Plus Admission on Non-Monetary Outcomes



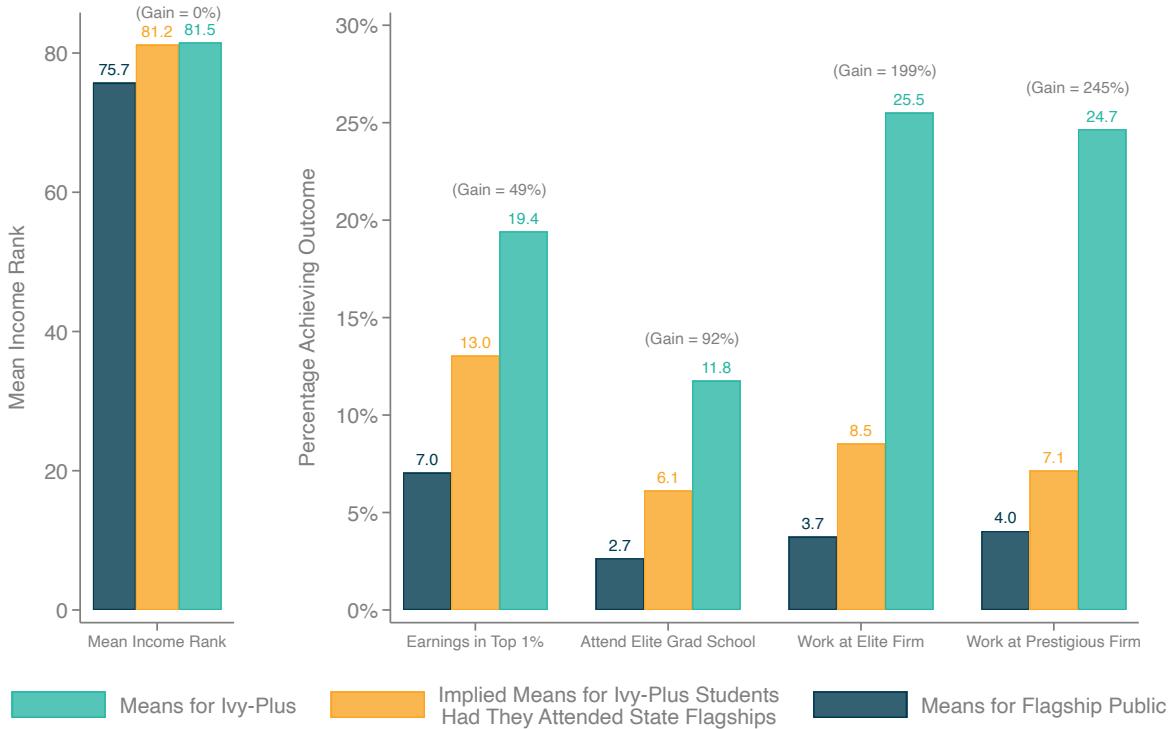
Notes: Figures 11 replicates Figure 8 using non-monetary outcomes: attending an elite graduate school at age 28 (Figure 11a), working at an elite firm at age 25 (Figure 11b), and working at a prestigious firm (Figure 11c). Elite graduate schools are defined as Ivy-Plus institutions, as well as UC-Berkeley, UCLA, UCSF, University of Michigan, and University of Virginia. Elite firms are defined as firms that employ the highest share of Ivy-Plus graduates relative to graduates of flagship public colleges (leaving out the individual's own college). Prestigious firms are identified based on the same ratio, controlling for the share of individuals with income in the top 1% at that firm. See Section 2 for additional details on the definitions of these variables.

Figure 12: Causal Effects of Ivy-Plus Attendance: Matriculation Design



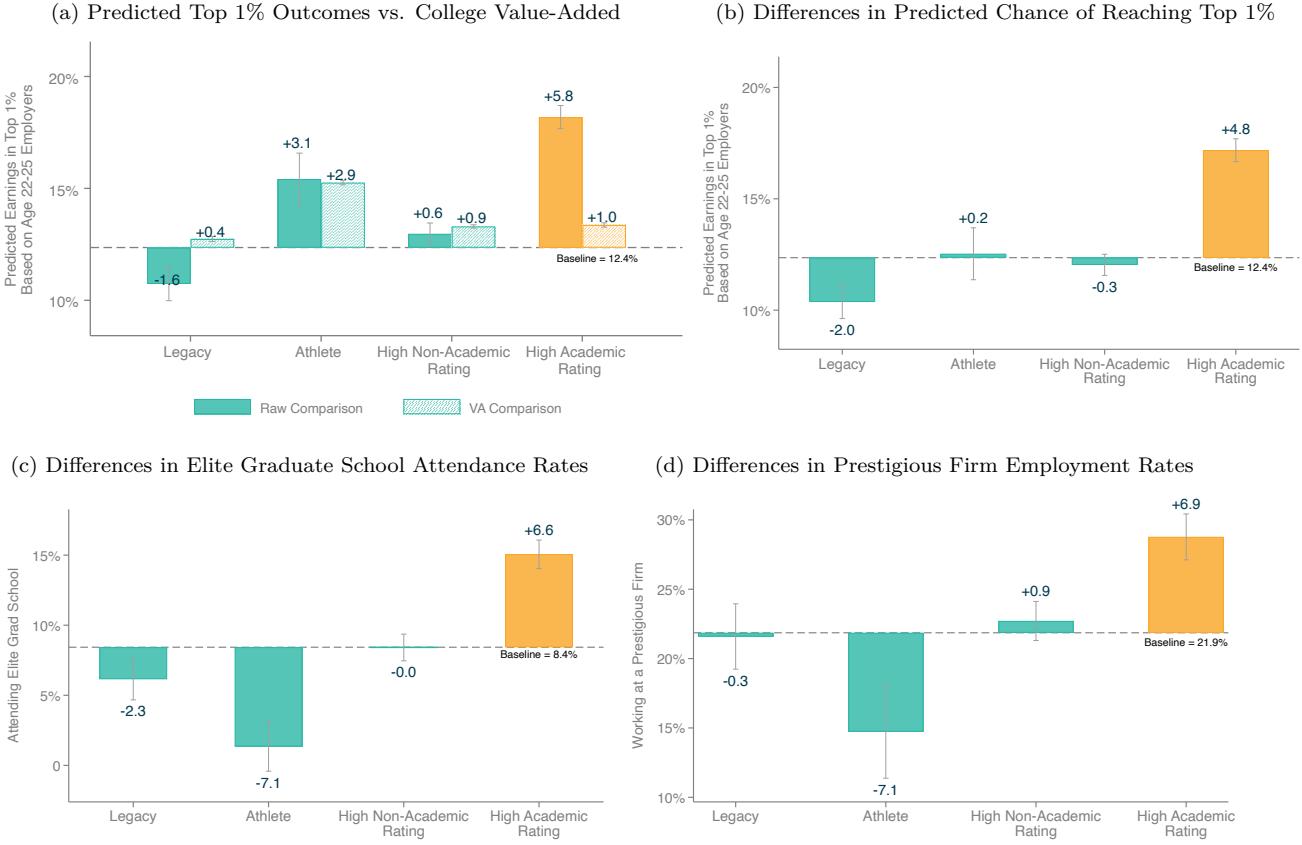
Notes: This figure presents estimates of colleges' causal effects based on variation in where students choose to attend conditional on the set of colleges to which they were admitted. The first three panels are scatter plots of colleges' causal effects vs. observational value-added estimates. Causal effects are estimated using regressions of outcomes on indicators for school attended with fixed effects for the exact set of schools to which the student is admitted (among the set of schools plotted in each panel) as controls. Observational value-added estimates are based on OLS regressions of outcomes on fixed effects for the college students attend, controlling for parental income, SAT scores, race, gender, birth cohort, and home state, estimated using our pipeline analysis sample. The value-added estimates are normed such that the value-added of flagship public schools (listed in Appendix Table 1) is 0. Each dot represents a different college, except that we report the mean of the estimates for the Ivy-Plus colleges in our college-specific sample in a single point (denoted by a triangle), along with the point estimate and standard error for that causal effect. We also plot a best-fit line based on a regression on the plotted points, as well as the slope and standard error for that line. Figure 12a presents estimates from this design for the predicted top 1% based on ages 22-25 firms outcome, using only the Ivy-Plus and flagship public colleges in our college-specific sample. Figure 12b replicates Figure 12a, but additionally includes data from all other schools in the UC system, all schools in the CSU System, and all other 4-year public schools in Texas. We use data from Texas for 1999-2008 school years to align with the sample used by Mountjoy and Hickman (2021). Figure 12c replicates Figure 12b, but using predicted mean income rank based on firm at ages 22-25 as the outcome variable. Figure 12d reports the causal effect of attending Ivy-Plus colleges (relative to the flagship public schools) as in Figure 12b, but separately for students from each of eight parent income bins; the dashed lines present 95% confidence intervals. All estimates in this figure are based on our college-specific analysis sample.

Figure 13: Differences in Outcomes at Ivy-Plus vs. Flagship Public Colleges: Causal Effects vs. Selection



Notes: Figure 13 shows how much of the difference in observed post-college outcomes between Ivy-Plus and flagship public students is due to causal effects of colleges vs. selection across colleges. In each triplet of bars, the first bar plots mean observed outcomes for the nine flagship public schools listed in Appendix Table 1. The last of the three bars plots mean observed outcomes for the Ivy-Plus colleges listed in Appendix Table 1. The middle bar shows implied means for Ivy-Plus students had they attended state flagships by subtracting treatment effect estimates from the mean observed outcomes for Ivy-Plus attendees. For elite graduate school attendance, elite firm employment, and prestigious firm employment, we estimate the causal effect of Ivy-Plus attendance by multiplying the waitlist TOT effect on the relevant outcome (as estimated in Figure 8 or Figure 11) by the ratio of the difference in mean observational value-added between the Ivy-Plus and nine flagship public schools and the waitlist TOT effect on value-added of college attended (for the relevant variable). For mean income ranks and earnings in top 1% at 33, we estimate the treatment effect by rescaling the difference in the observational VA estimates at age 33 by the ratio of the waitlist design to observational VA estimate for predicted incomes based on the ages 22-25 employers, which we are able to estimate with greater precision. The difference between the first and second bars in each triplet can be interpreted as the part of the difference in observed outcomes between Ivy-Plus and state flagship students that is due to selection, while the difference between the second and third bars is the causal effect of Ivy-Plus attendance. See notes to Figure 8 and Figure 11 for more detail on the variables, sample, and waitlist-based estimates of the TOT effects.

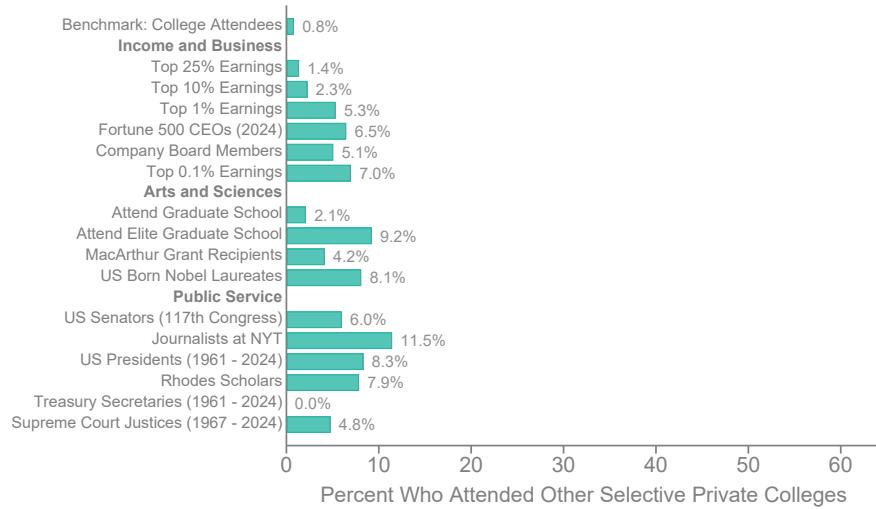
Figure 14: Post-College Outcomes by Application Credentials Among Ivy-Plus Applicants



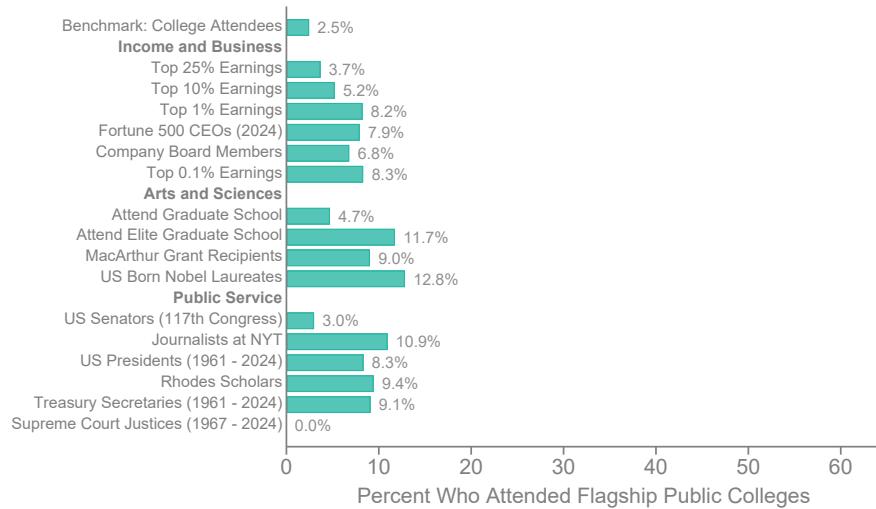
Notes: In Figure 14a, the bars on the left in each pair report estimates from regressing the predicted probability of reaching the top 1% based on ages 22-25 employers on four explanatory variables: indicators for whether a student is a legacy, is a recruited athlete, has a high non-academic rating, and has a high academic rating. The sample consists of students either admitted or offered a place on the waitlist at the Ivy-Plus college with the most granular ratings data in our sample. We plot the regression coefficients plus the baseline rate for the outcome in the sample, defined as the mean of the outcome non-legacy, non-athlete applicants with low academic and non-academic ratings. In the bars on the right in each pair, we replace the dependent variable with the observational value-added of college attended multiplied by the ratio of the waitlist-design treatment effect estimate to the observational VA estimate reported in Columns 1 and 5 of Appendix Table 8. Figure 14b plots the difference between the Raw Outcome Comparison and VA Comparison in Figure 14a for the four explanatory variables plotted in Figure 14a. These estimates show the difference in outcomes for applicants by their credentials, netting out differences in the value-added of the college they attend. See Appendix Figure 36 for an illustration of the levels underlying Panels A and B for those with low vs high non-academic ratings. Figure 14c and Figure 14d replicate Figure 14b using an indicator for attending an elite graduate school at age 25 and working at a prestigious firm at age 25 as the dependent variables.

Appendix Figure 1: Share of Individuals in Leadership Positions who Attended Non-Ivy-Plus Colleges

(a) Other Highly Selective Private Colleges



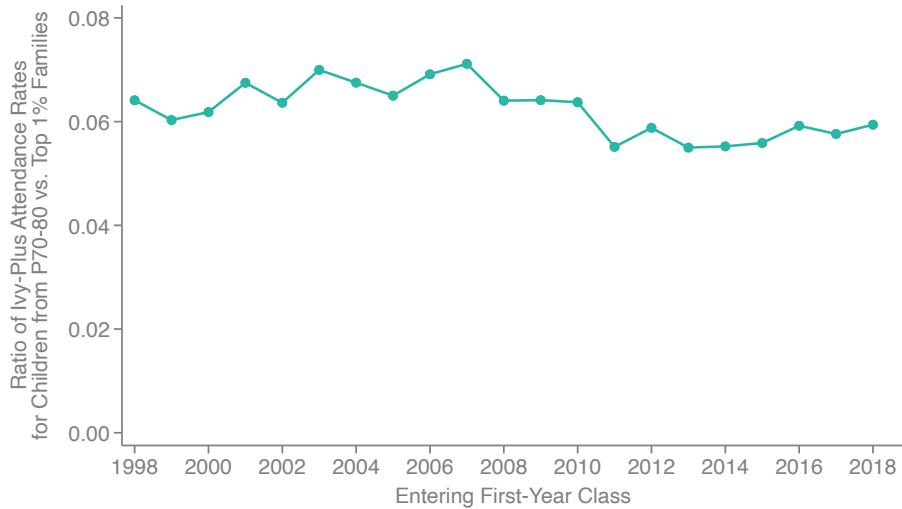
(b) Flagship Public Colleges



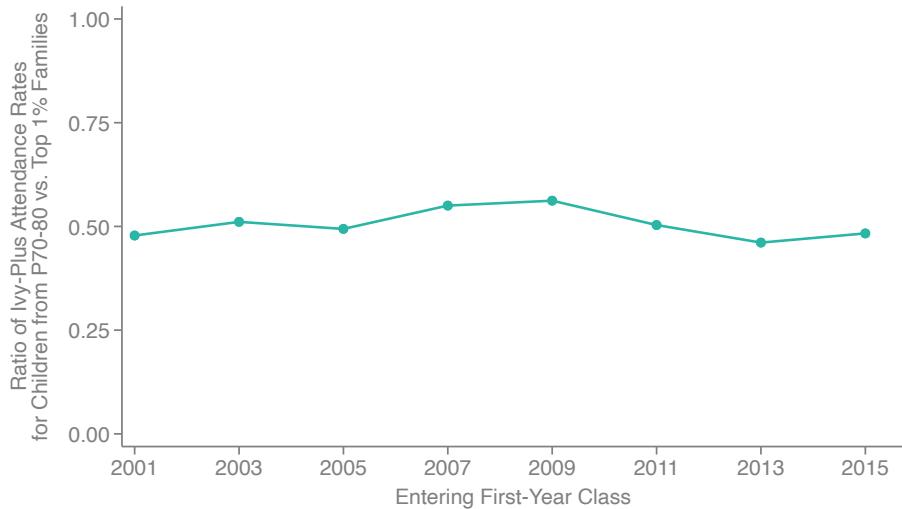
Notes: This figure replicates Figure 1, but showing the share who attended one of the other most selective private colleges (in Panel A) or one of the nine most selective flagship public colleges (in Panel B) listed in Appendix Table 1. See notes to Figure 5 for further details and Appendix A for the definitions and sources for each outcome variable.

Appendix Figure 2: Trends in Ivy-Plus Attendance Rates for Children from Middle Class vs. Top 1% Families

(a) Ratio of P70-80 to Top 1% Ivy-Plus Attendance Rates, 1998-2018

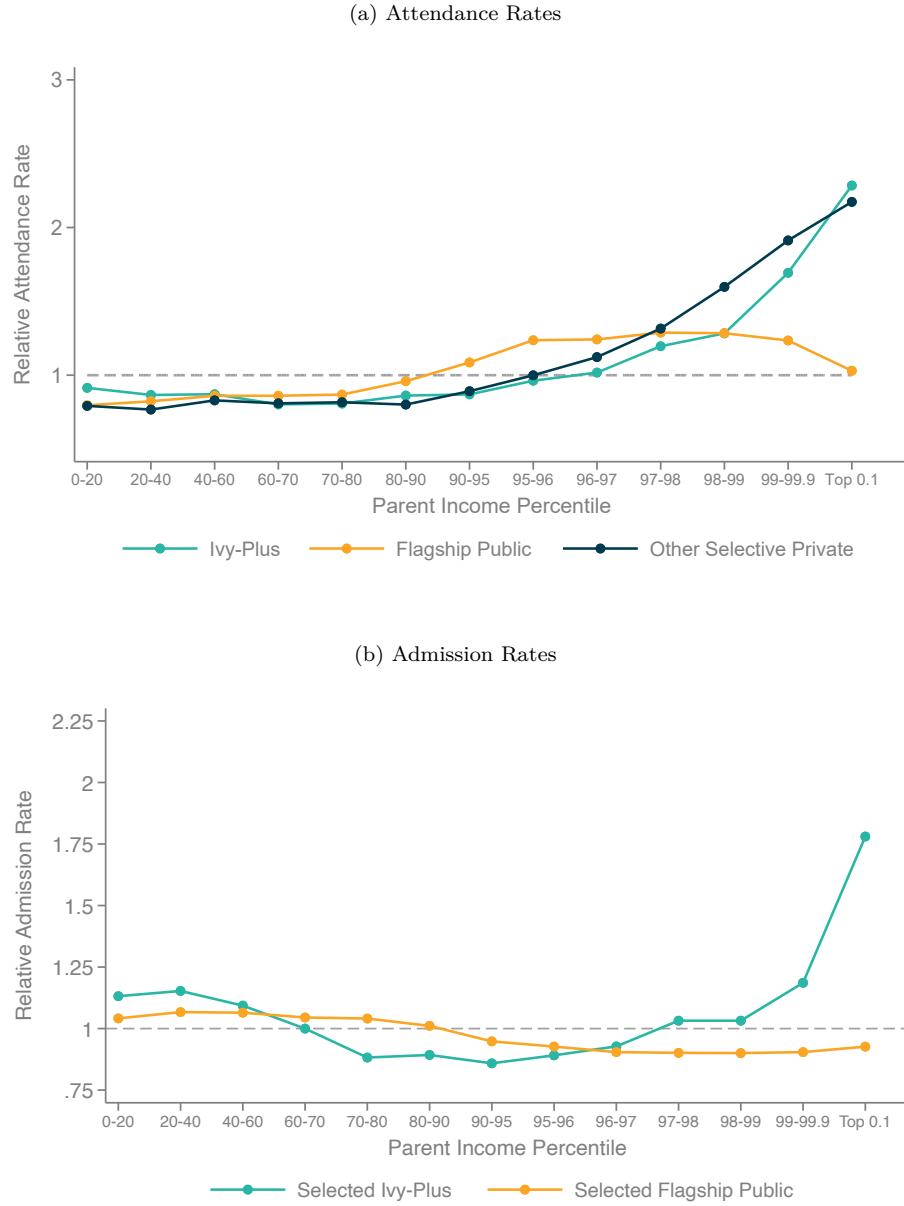


(b) P70-80 / Top 1% Ivy-Plus Attendance Rates Controlling for Test Scores, 2001-2015



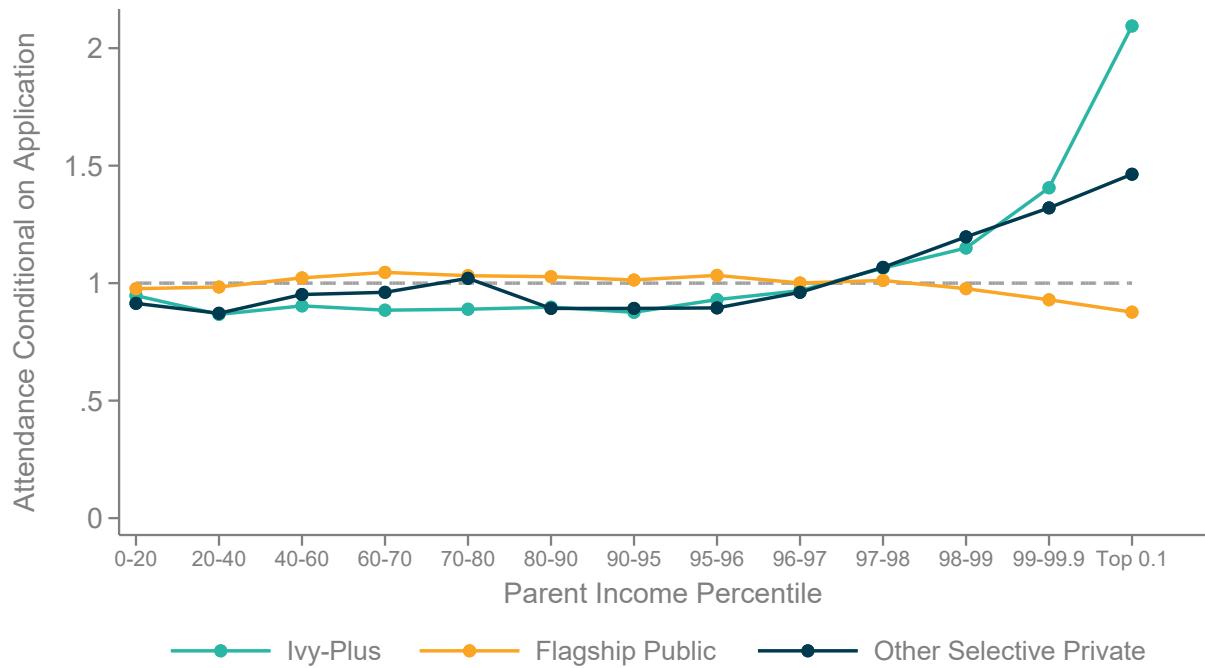
Notes: Appendix Figure 2 plots the ratio of the share of students with parental incomes between the 70th and 80th percentile who attend Ivy-Plus colleges to the share of students with parental incomes in the top 1% who attend Ivy-Plus colleges by year for students who turn 18 in the years from 1998-2018. Appendix Figure 2b plots the same ratio reweighting on test scores to match the distribution of test scores attending each Ivy-Plus college in each year, as in Figure 2b.

Appendix Figure 3: Attendance and Admission Rates by Parental Income, Controlling for Test Scores and Race



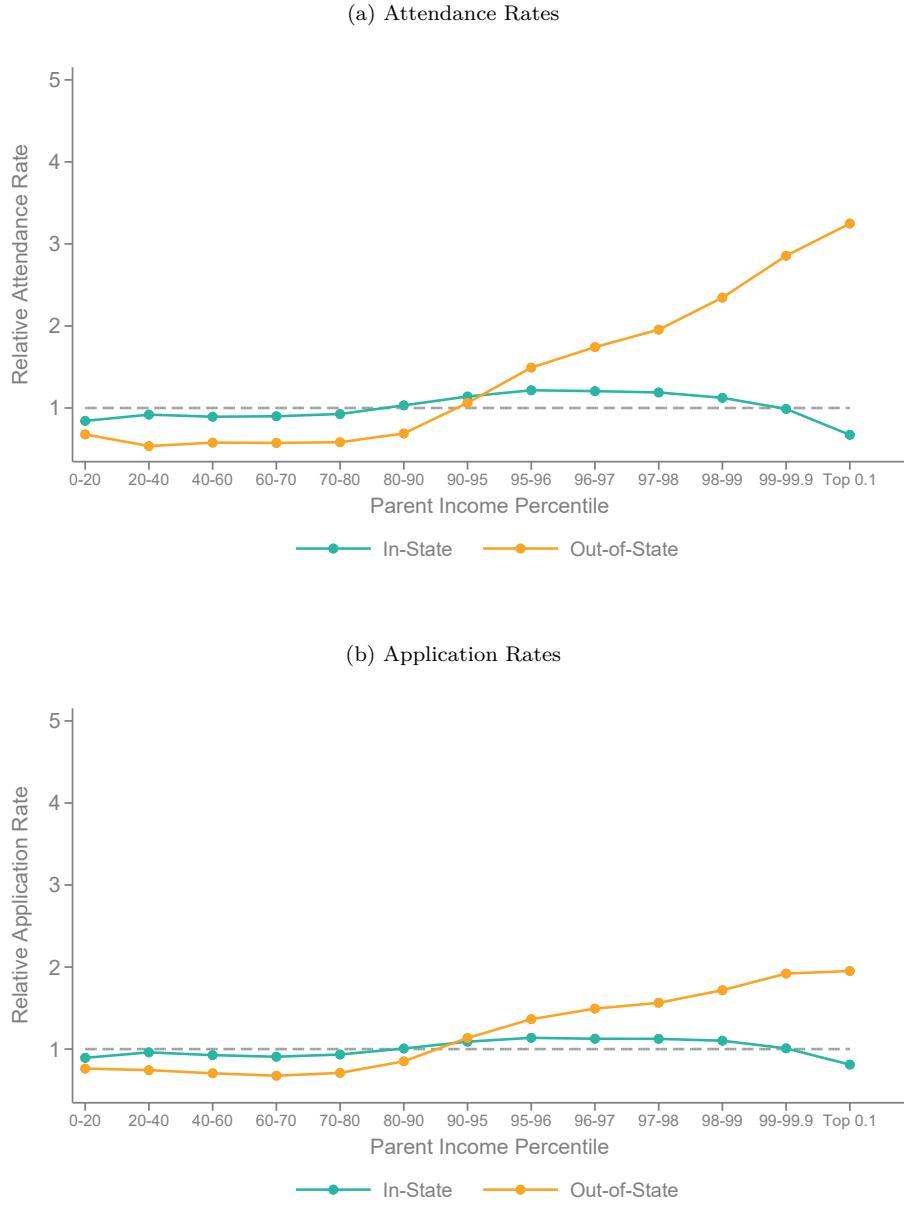
Notes: Appendix Figure 3a replicates Figure 2b, but reweighting so that the joint distribution of race/ethnicity (categorized as described in Section 2) and test scores within each parent income bin matches the distribution for attending students overall. Appendix Figure 3b replicates Figure 3b, similarly reweighting on both test scores and race.

Appendix Figure 4: Attendance Rates Conditional on Application



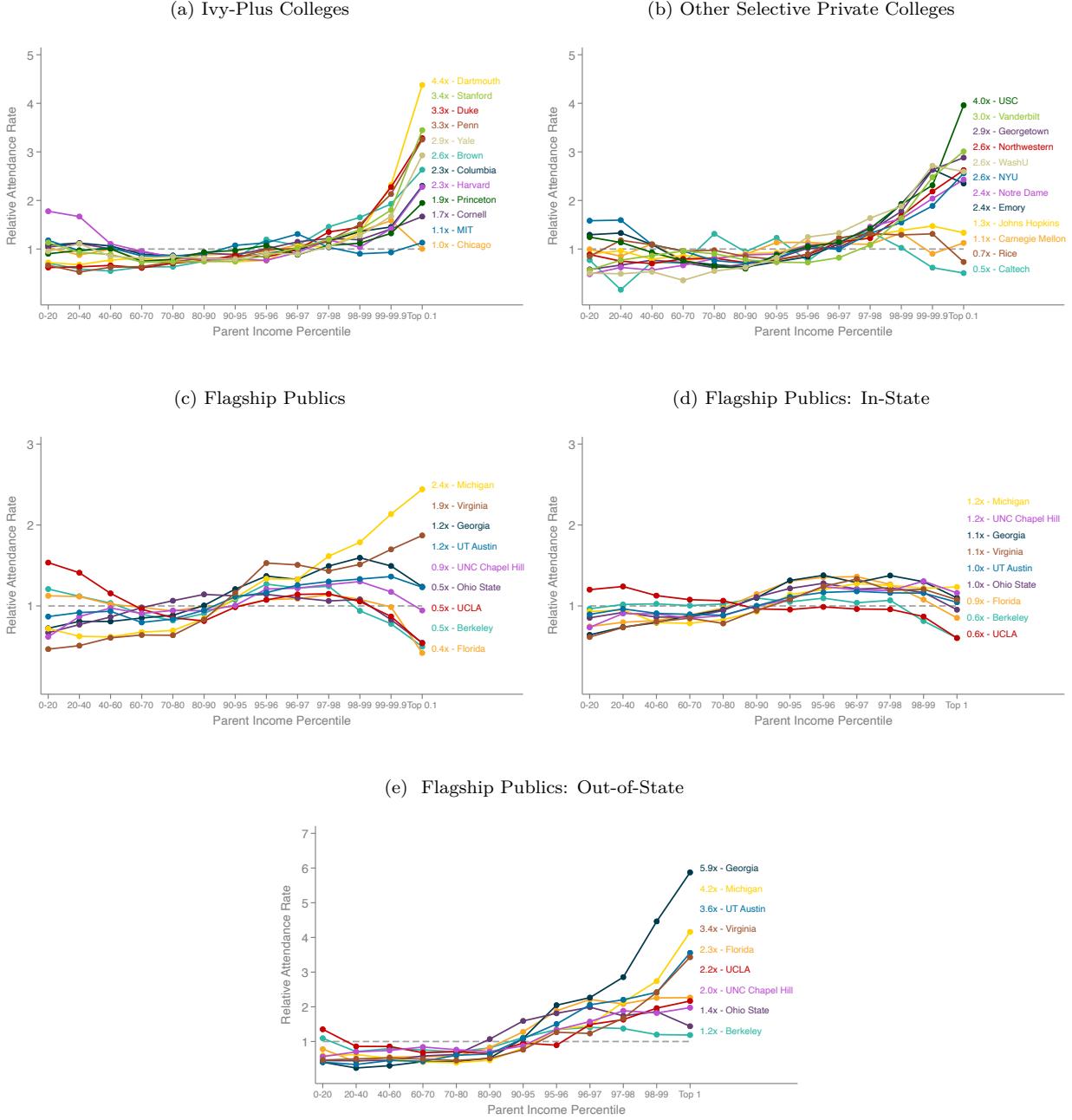
Notes: Appendix Figure 4 replicates Figure 2b but with attendance rates conditional on application, defined as the ratio of attendance rates to application rates. See Appendix Figure 8 for analogous data for specific colleges.

Appendix Figure 5: Flagship Public College Attendance and Application Rates by Parental Income and In-State Status, Controlling for Test Scores



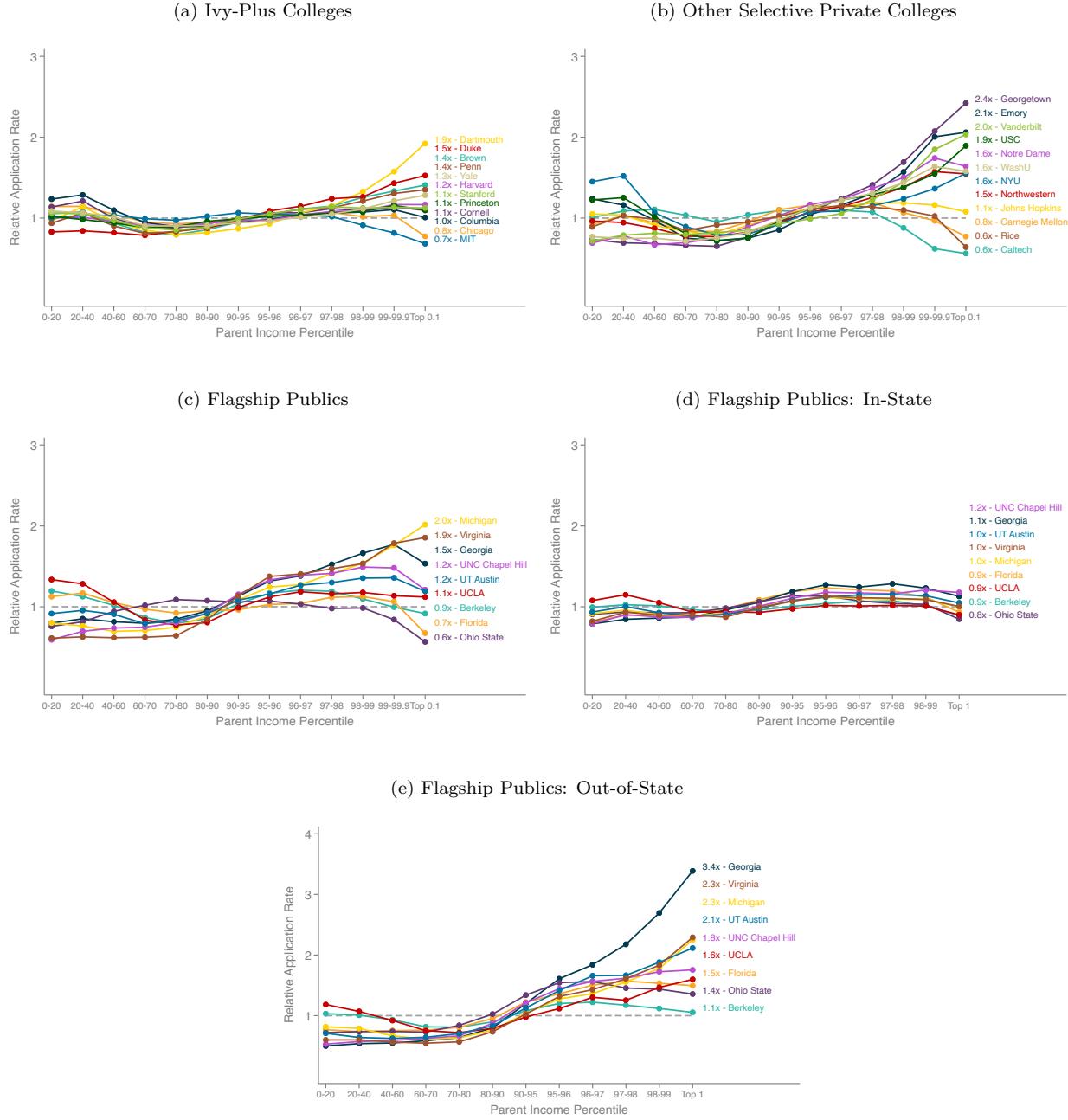
Notes: Appendix Figure 5a replicates the estimates from Figure 2b for the nine flagship public universities listed in Appendix Table 1, splitting students into those living in state vs. out of state. We measure in-state status using the students' state of residence when they take a standardized test. Appendix Figure 5b replicates Appendix Figure 5a replacing attendance rates with application rates. Application rates are predicted using score sending data as described in Appendix C.

Appendix Figure 6: Attendance Rates by Parental Income and College, Controlling for Test Score



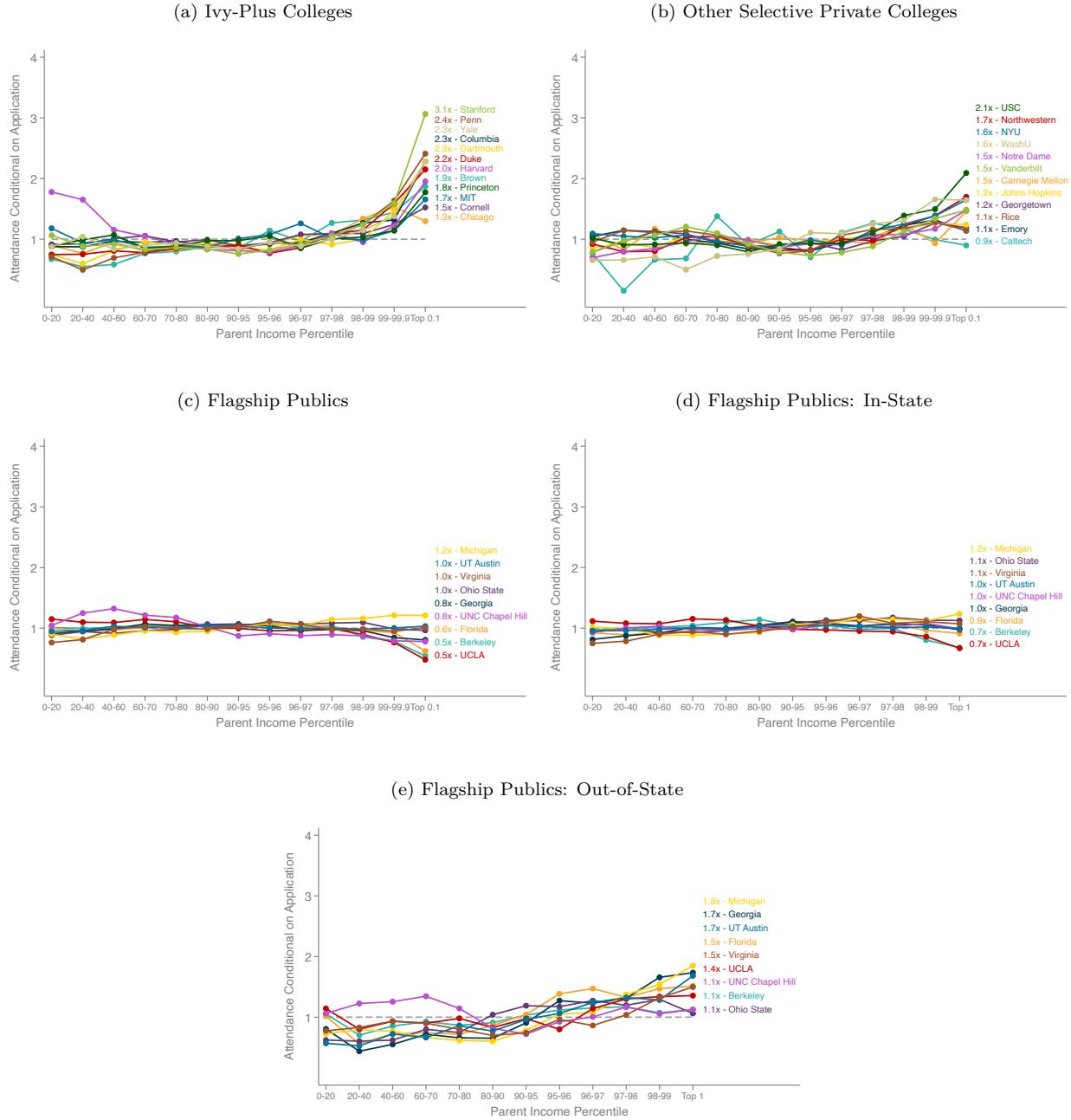
Notes: This figure replicates the estimates in Figure 2b and Appendix Figure 5a, but separately by college for all colleges listed in Appendix Table 1 using our pipeline analysis sample. Appendix Figure 6a plots relative attendance rates for each Ivy-Plus college; Appendix Figure 6b plots relative attendance rates for each other highly selective private college; Appendix Figure 6c plots relative attendance rates for of the flagship public universities pooling in-state and out-of-state students, while Appendix Figures 6d and 6e repeat this for in-state and out-of-state students respectively. In all panels we follow a differential privacy approach and add random noise distributed $N(0, \frac{\Delta\theta}{\varepsilon})$ to each estimate, where $\Delta\theta$ is the global sensitivity of statistic θ and ε is the privacy loss parameter. Since the outcome is a binary variable, $\Delta\theta = \frac{1}{N}$ (where N is the number of observations behind a given estimate); we set $\varepsilon = 1$. See notes to Figure 2 and Appendix Figure 5 for details on variable definitions and methods.

Appendix Figure 7: Application Rates by Parental Income and College, Controlling for Test Score



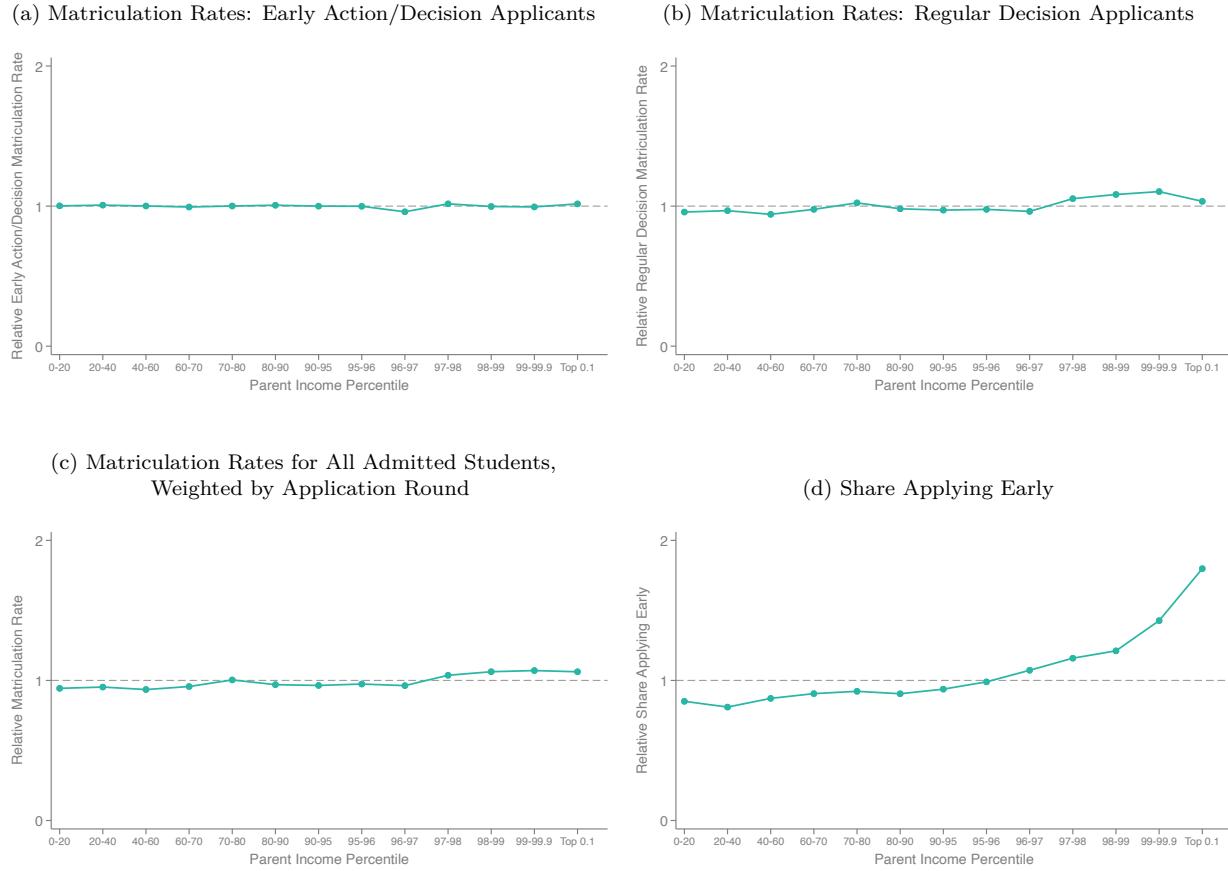
Notes: This figure replicates Appendix Figure 6, replacing attendance rates with application rates. Application rates are predicted using score sending data as described in Appendix C.

Appendix Figure 8: Attendance Rates Conditional on Application by Parental Income and College, Controlling for Test Score



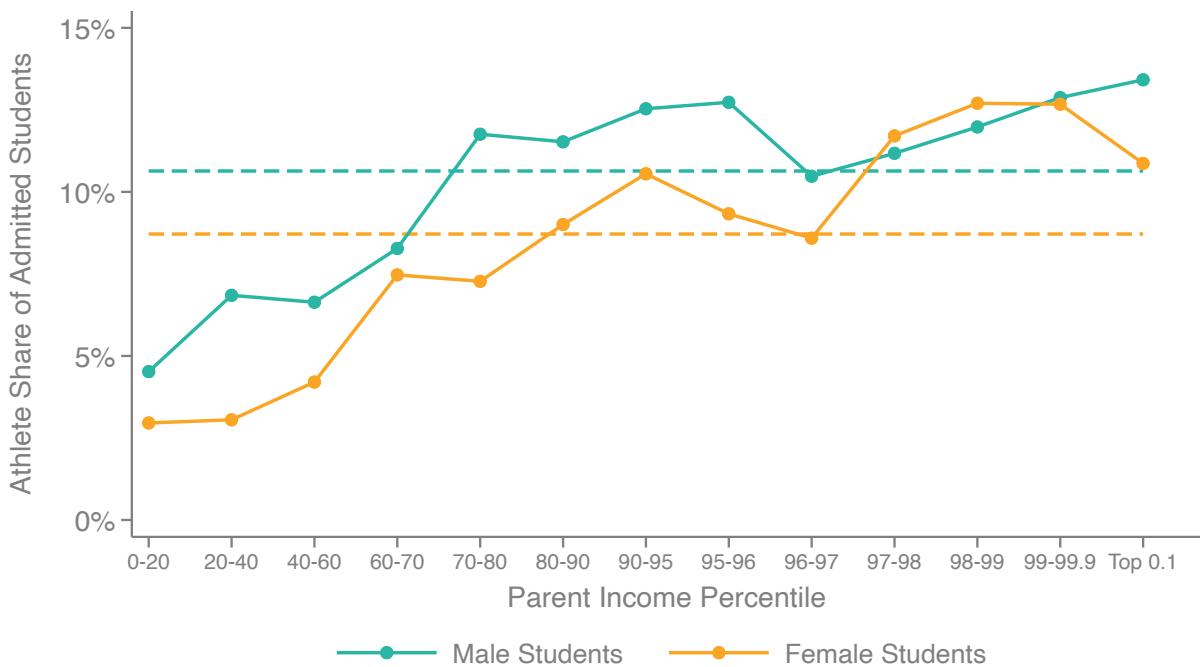
Notes: This figure replicates Appendix Figure 6, replacing attendance rates with the ratio of attendance rates to application rates. Application rates are predicted using score sending data as described in Appendix C.

Appendix Figure 9: Matriculation Rates at Ivy-Plus Colleges by Parental Income and Early vs. Regular Round Application, Controlling for Test Scores



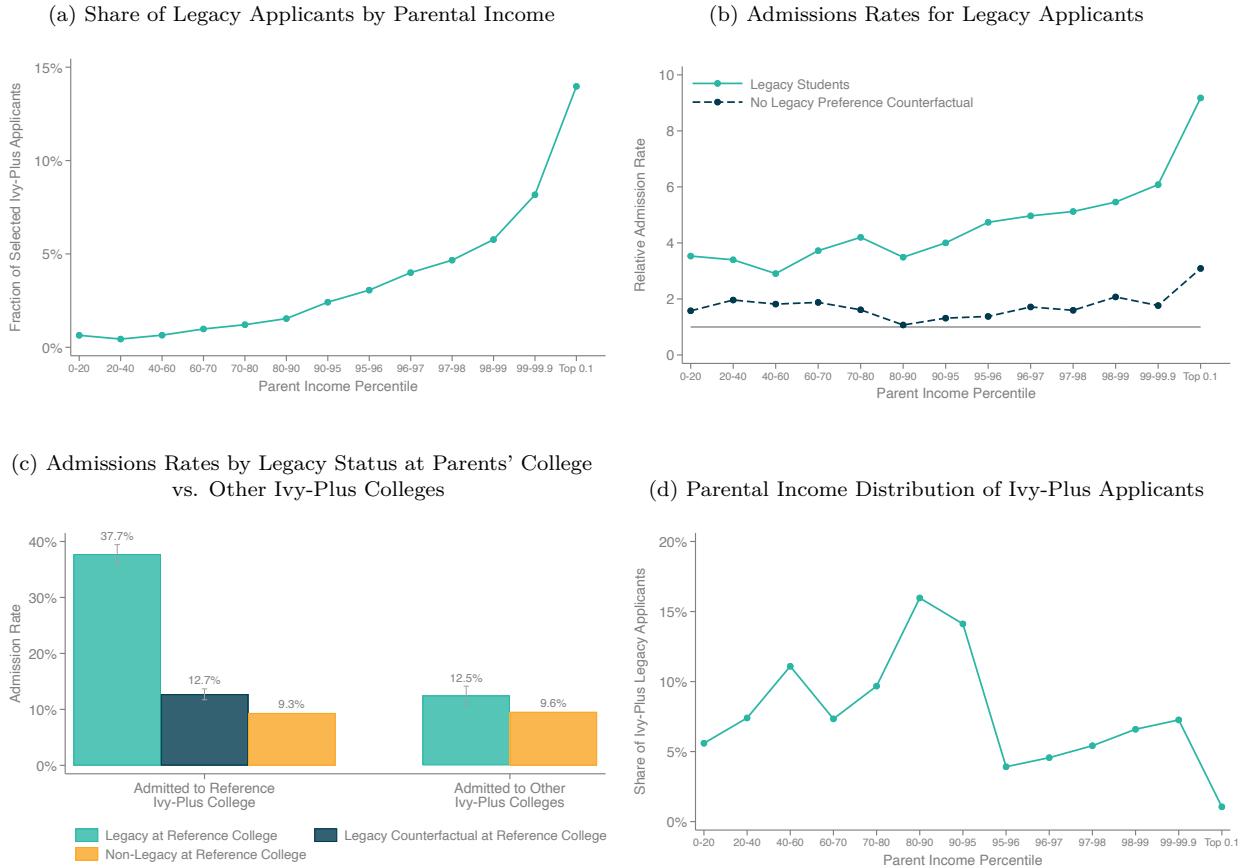
Notes: Appendix Figures 9a and 9b replicate Figure 3c but for students who were admitted to Ivy-Plus schools in the early action / early decision round or in the regular decision round, respectively. Appendix Figure 9c replicates Figure 3c, reweighting students on both test scores and application round to equalize the share of students in early vs. regular decision rounds across parent income bins. Appendix Figure 9d plots the share of early action/decision applicants by parental income reweighted by test score. Estimates are based on the college-specific sample of Ivy-Plus colleges; see Section 2 for details.

Appendix Figure 10: Share of Ivy-Plus Admitted Students who are Recruited Athletes, by Parental Income and Gender



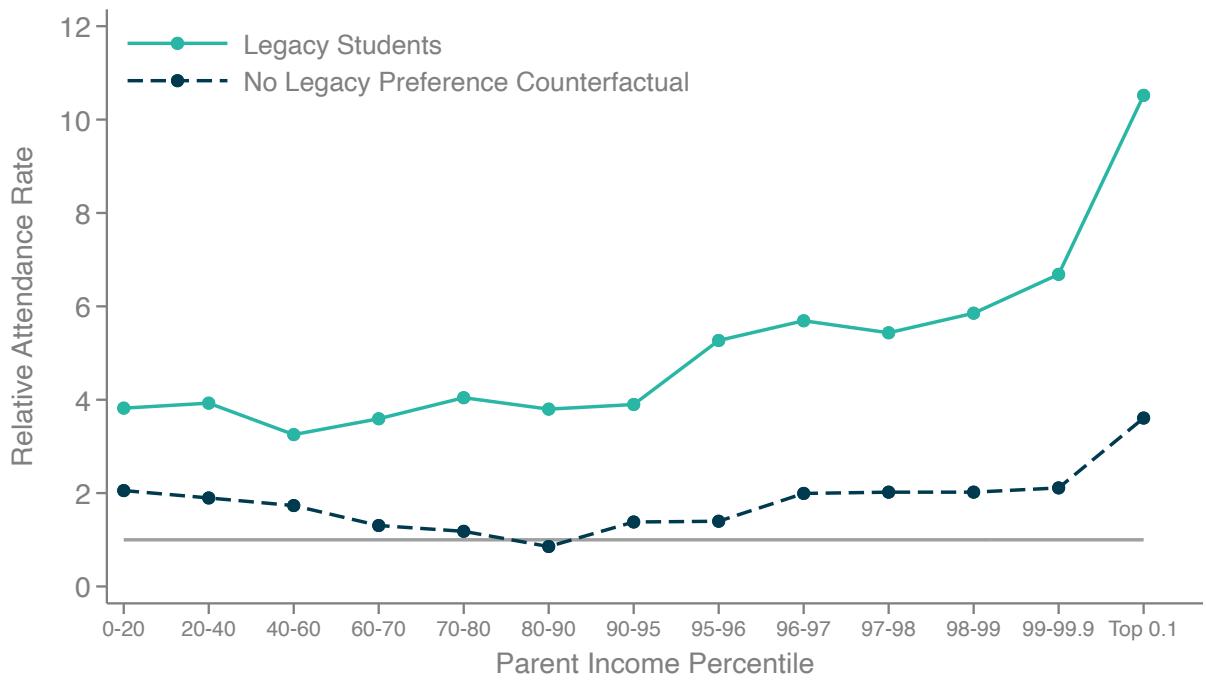
Notes: Appendix Figure 10 replicates Figure 4a separately by gender.

Appendix Figure 11: Ivy-Plus Legacy Applicant Shares and Admissions Rates by Parental Income



Notes: Appendix Figures 11a - 11c replicate Figures 5a - 5c without reweighting on test scores. Appendix Figure 11d plots the distribution of parent income among all applicants (without test score reweighting) in the same sample as in Appendix Figures 11a and 11b.

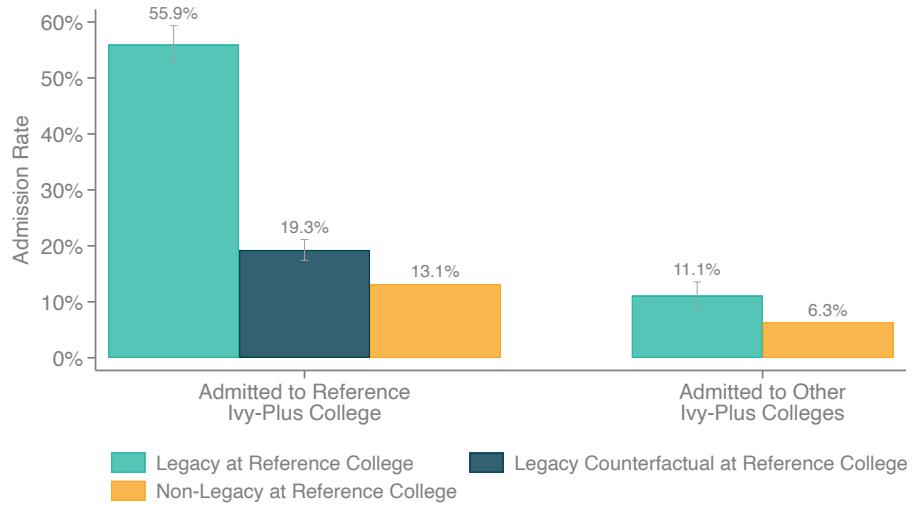
Appendix Figure 12: Actual vs. Counterfactual Attendance Rates for Ivy-Plus Legacy Students, by Parental Income



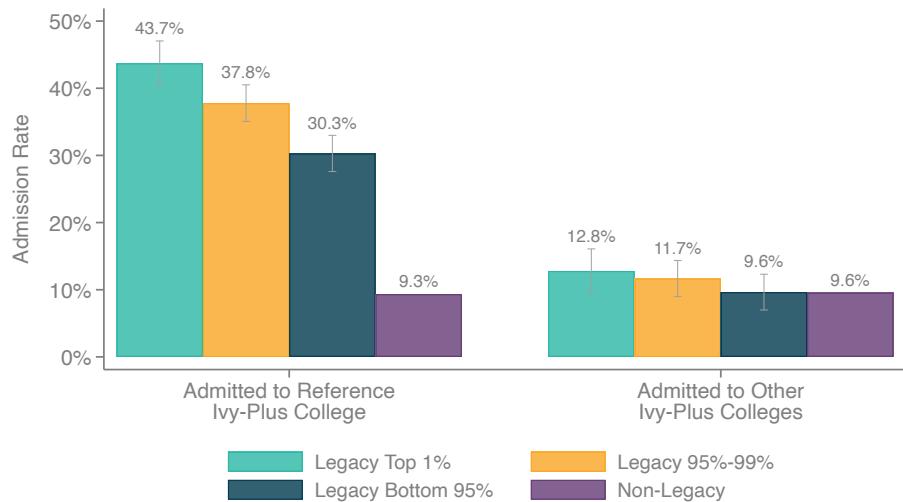
Notes: Appendix Figure 12 replicates Figure 5b, except that we plot actual and counterfactual attendance rather than admissions rates.

Appendix Figure 13: Admission Rate by Legacy Status

(a) Admission Rate by Legacy Status at Lower Ranked Colleges



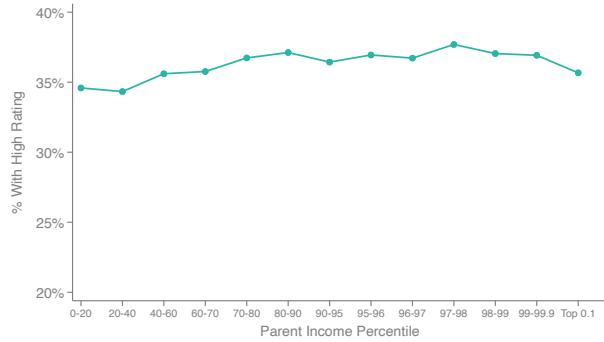
(b) Admission Rate by Legacy Status and Parental Income



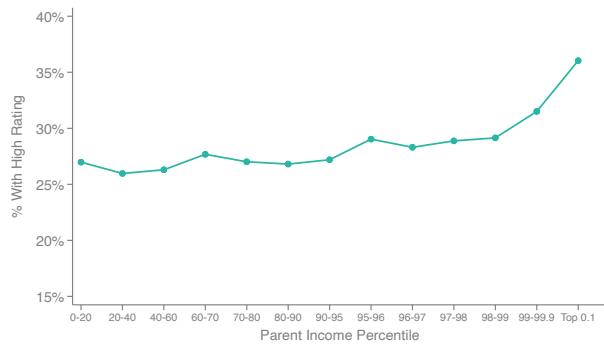
Notes: Appendix Figure 13a replicates Figure 5c, except that we limit the sample to reference colleges that are lower-ranked within the relevant pair of colleges (based on revealed preference), to mitigate concerns about strategic admission by other Ivy-Plus colleges. Appendix Figure 13b also replicates Figure 5c, but dividing legacy applicants into three groups based on parental income (top 1%, 95%-99%, and bottom 95%).

Appendix Figure 14: Admissions Office Ratings of Applicants by Parent Income: Sensitivity Analysis

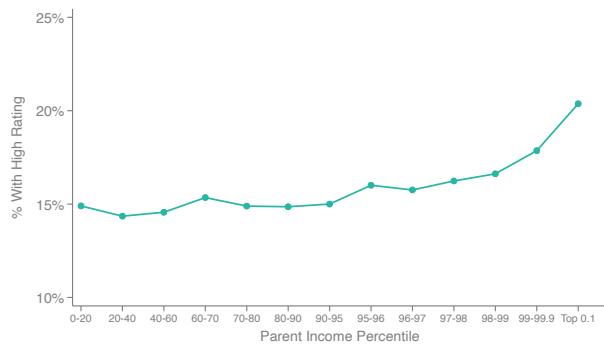
(a) Academic Ratings, Pooling Multiple Ivy-Plus Colleges



(b) Coarse Non-Academic Ratings, Focal Ivy-Plus College

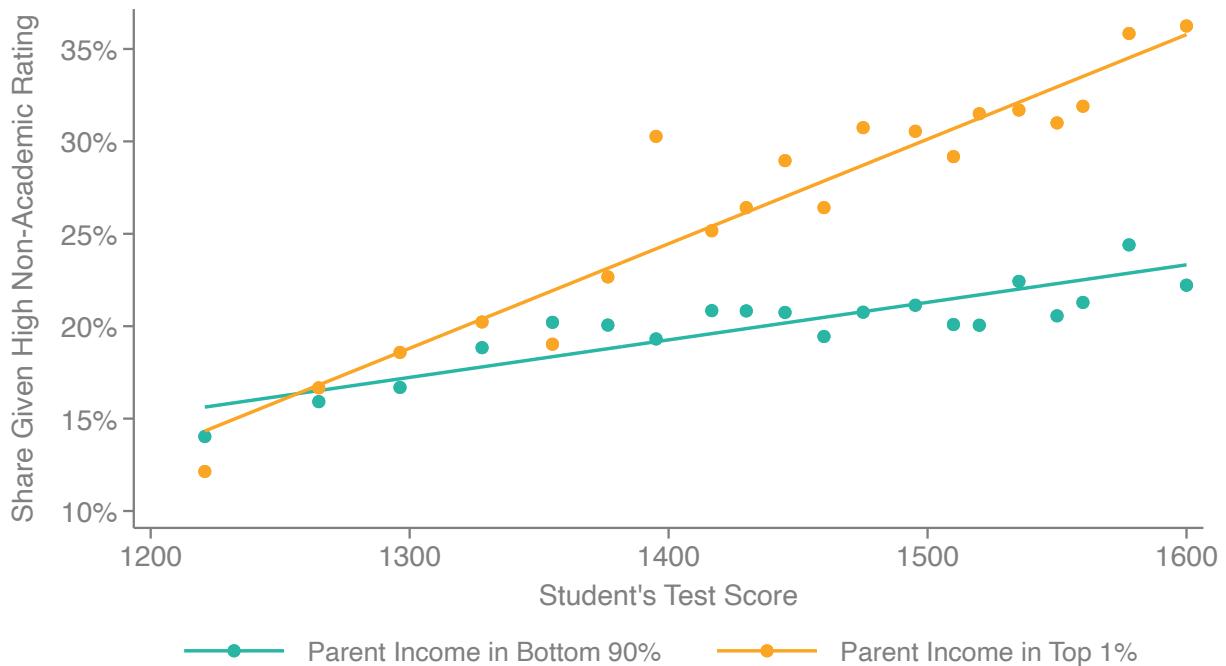


(c) Coarse Non-Academic Ratings, Pooling Multiple Ivy-Plus Colleges



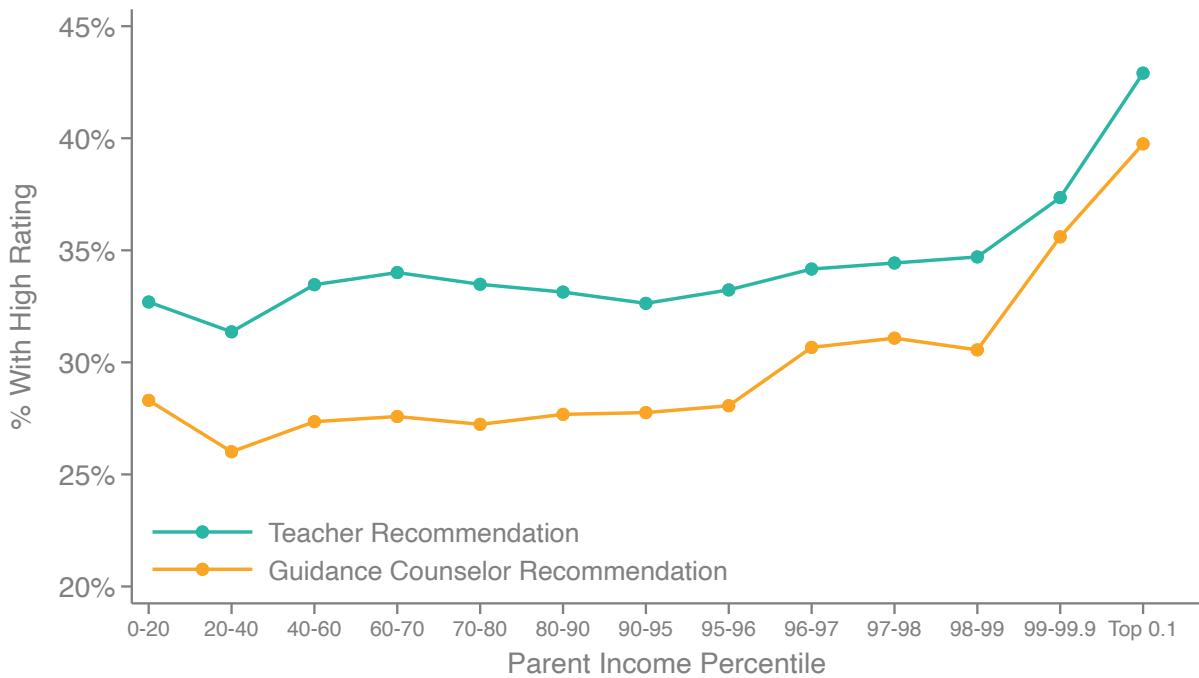
Notes: Appendix Figure 14 replicate the results on Figures 6a and 6b using a broader set of Ivy-Plus colleges. Panel A replicates Figure 6a with data from multiple Ivy-Plus colleges. Panel B replicates Figure 6b with data only from the Ivy-Plus college used in Figure 6b, but coarsening the measurement of non-academic rating to match the measurements available in datasets from other Ivy-Plus colleges. Panel C then replicates Panel B using data from all the Ivy-Plus colleges in our college-specific sample for which we have ratings information.

Appendix Figure 15: Non-Academic Ratings vs. Test Scores, by Parental Income



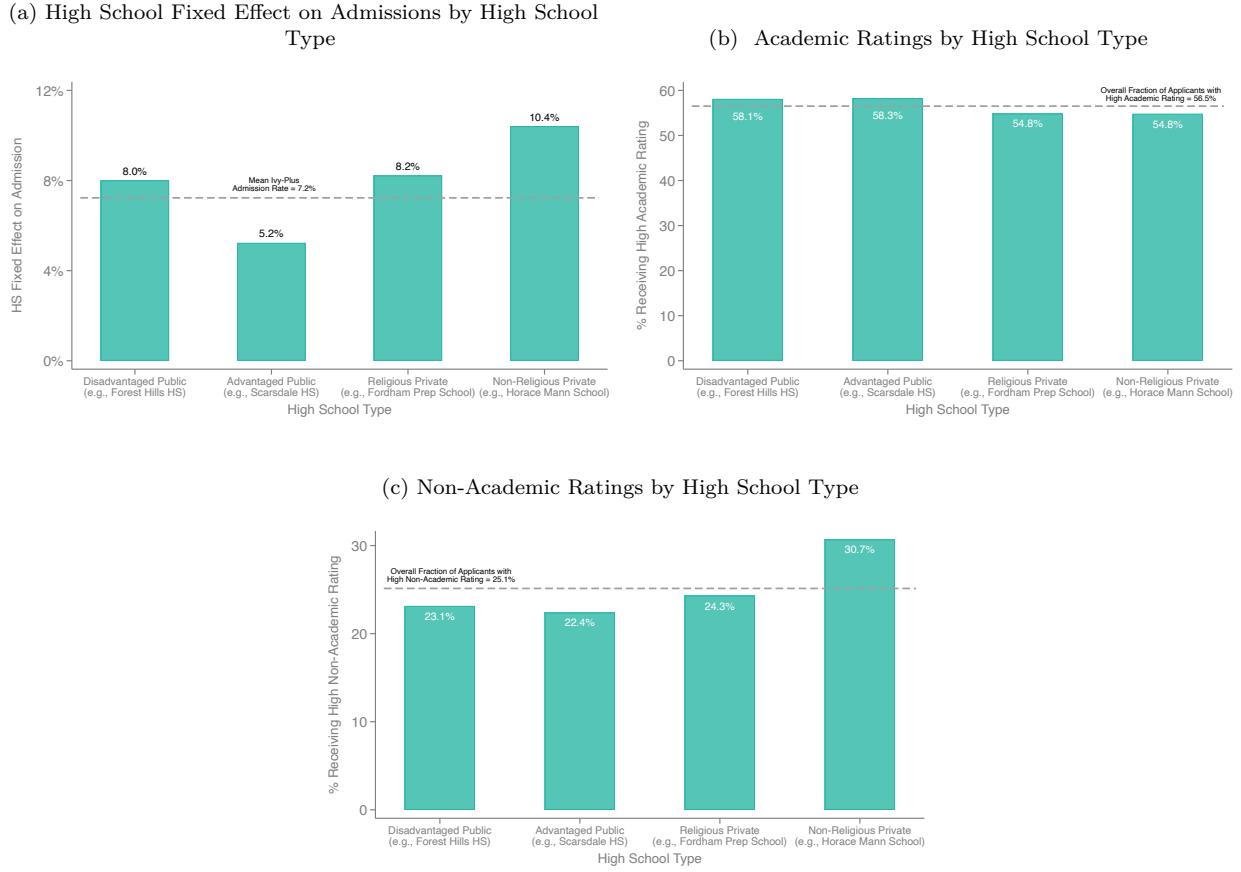
Notes: Appendix Figure 15 presents a binned scatter plot of the share of applicants given high non-academic ratings by student test score ventile, separately for students with parents in the bottom 90 percent of the income distribution vs. those with parents in the top 1 percent. Estimates are based on applicants to the Ivy-Plus college for which we have the most granular ratings data. The sample includes all applicants with SAT scores greater than 1200 (or, equivalently, ACT scores greater than 21), excluding recruited athletes, legacy students, and faculty children.

Appendix Figure 16: Teacher and Guidance Counselor Ratings by Parental Income, Controlling for Test Scores



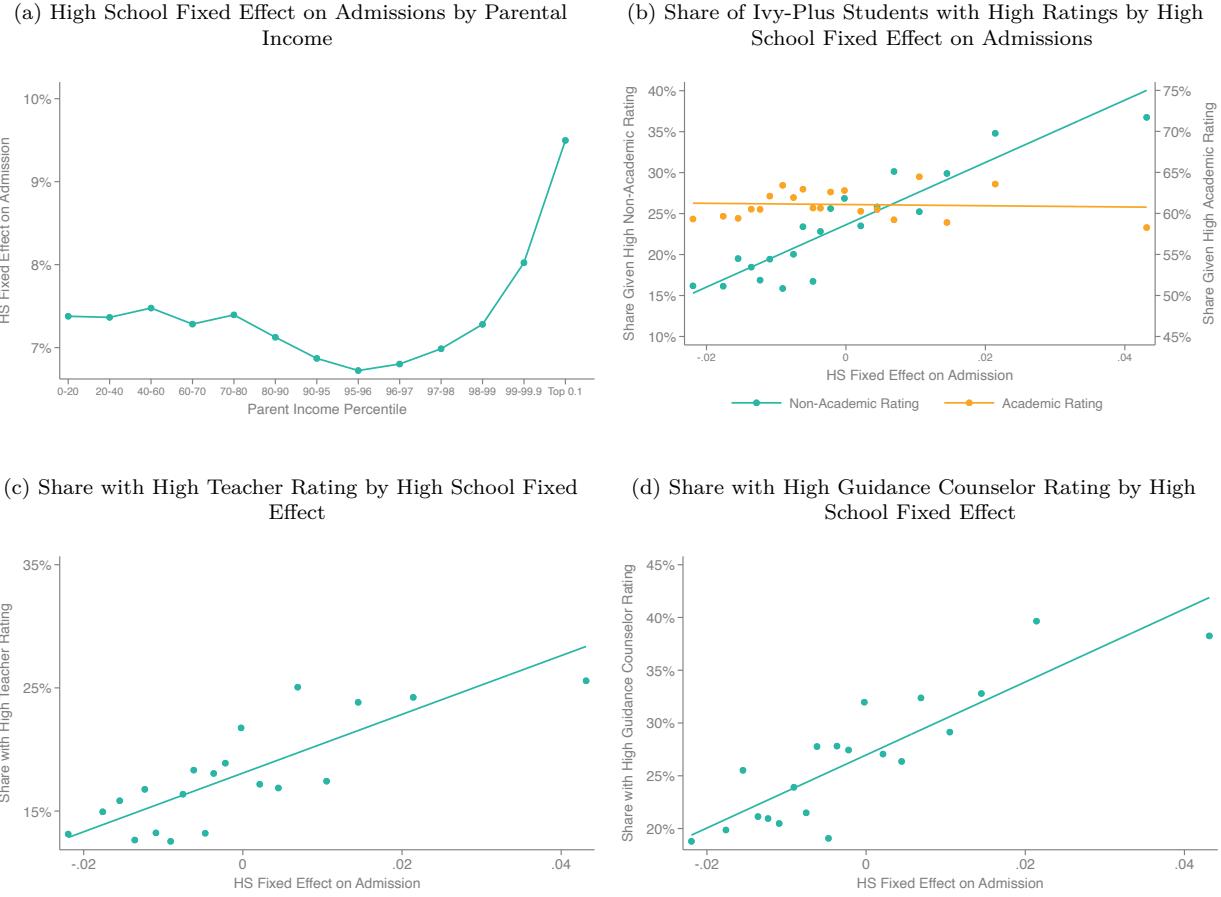
Notes: Appendix Figure 16 plots the proportion of Ivy-Plus applicants receiving high admissions ratings for letters of recommendation from teachers (green) and school guidance counselors (orange), following the same process as Figure 6a and Figure 6b.

Appendix Figure 17: Admissions and Ratings by High School Type



Notes: Appendix Figure 17 plots differences in admissions and student ratings by high school type. We first estimate high school fixed effects on Ivy-Plus admissions using a linear probability model omitting recruited athletes, legacy applicants, and faculty children, focusing on high schools with at least 40 Ivy-Plus applicants across the years of our sample. The admissions model includes fixed effects for exact SAT/ACT score, fixed effects for the interaction of race, gender, and parent income, and fixed effects for each high school. We estimate a jackknife fixed effect for each student i that excludes his/her own observation from the high school fixed effect estimate. Appendix Figure 17a plots the mean high-school admissions fixed effect (adding back the mean admissions rate) for four mutually exclusive sets of high schools. We break public high schools into two groups based on their percentile on high school challenge indicators that capture educational opportunities or disadvantages in the high school environment, variables that feed into the CollegeBoard Landscape tool. We classify high schools that fall in the top 20% of this index of advantage as “advantaged”. We give examples of high schools in each of the four groups from the New York City metro area for illustrative purposes; see Appendix Table 17 for analogous examples from other metro areas. Appendix Figures 17b and 17c plot the proportion of non-recruited-athlete applicants who receive high academic and non-academic ratings respectively by high school type, controlling for test scores. We regress an indicator for receiving a high rating on indicators for the four high school types and a quintic in test scores and plot the coefficients on the indicators (normalized so that the weighted average of the four coefficients matches the unconditional mean share of students who receive a high rating). Estimates are based on applicants to the Ivy-Plus college for which we have the most granular ratings data.

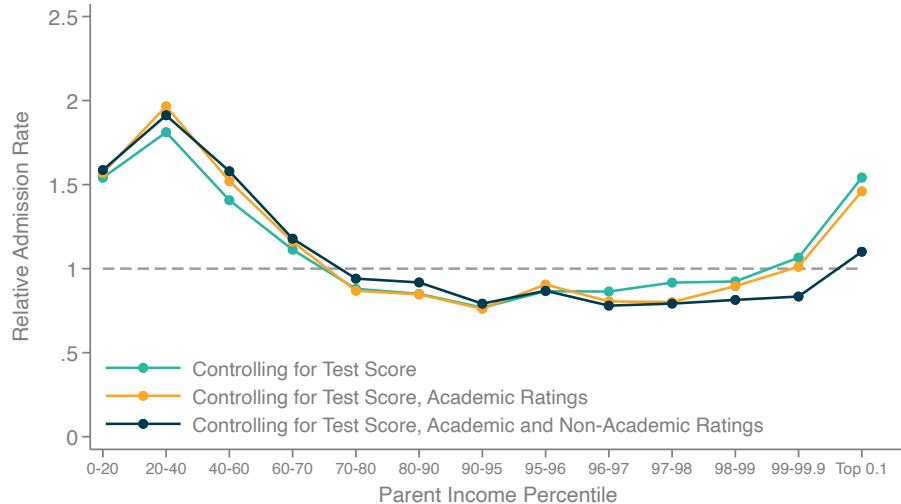
Appendix Figure 18: Ratings vs. High School Fixed Effects on Admissions



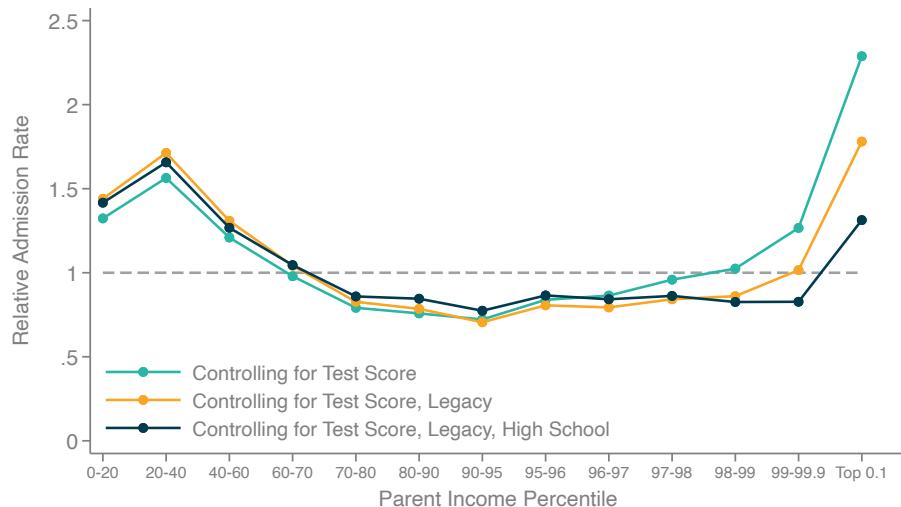
Notes: Appendix Figure 18 plots differences in student ratings by high school fixed effect. Appendix Figure 18a plots the mean high school fixed effect on admissions by parental income bin. Appendix Figure 18b is a binned scatterplot showing the share of applicants with high academic and non-academic ratings (as defined in Figure 6); to adjust for attenuation bias, in this panel, we shrink each high school fixed effect estimate towards zero by multiplying it by its reliability. To calculate reliability, we estimate the aggregate noise variance for the high school fixed effects as the average of the standard errors squared, and the signal variance as the total variance minus the aggregate noise variance; the reliability for each fixed effect is the signal variance divided by the sum of the signal variance plus the standard error of the school-specific estimate squared. All estimates are based on data from the Ivy-Plus college in our college-specific sample that records the most granular ratings information. Appendix Figures 18c and 18d replicate Appendix Figure 18b using the share of students with high ratings of teacher recommendation and guidance counselor letters respectively. See the notes to Appendix Figure 17 for a description of how the high school fixed effects are constructed.

Appendix Figure 19: Admissions Rates by Parental Income, Controlling for Ratings and High School

(a) Effect of Controlling for Ratings



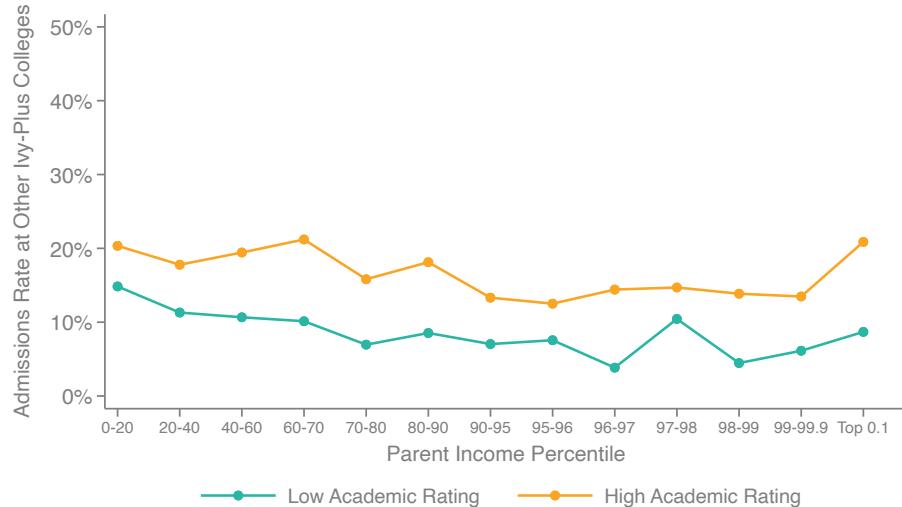
(b) Effect of Controlling for High School



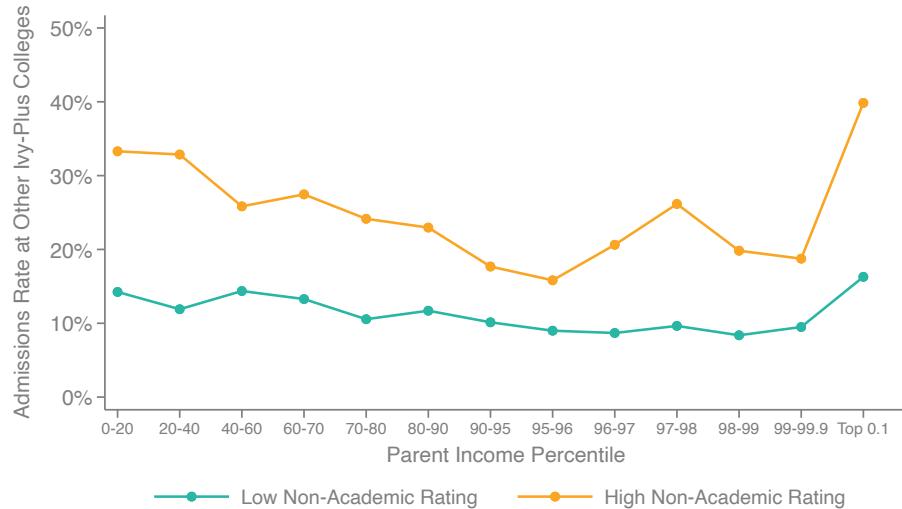
Notes: Appendix Figure 19a plots admissions rates for non-legacy, non-recruited-athlete applicants to the Ivy-Plus college for which we have the most granular ratings information with three set of weights. The green line reweights on test score, so that the distribution of test scores within each parent income bin matches that of attending students, as in Figure 2b. The orange line reweights on the joint distribution of test score and academic rating. The dark blue line reweights on the joint distribution of test score, academic rating, and non-academic rating. Appendix Figure 19b plots admissions rates for non-recruited-athletes at the same Ivy-Plus college by parental income bin reweighting on test scores (green line), adding regression controls for legacy status (orange line), and finally adding high school fixed effects (dark blue line).

Appendix Figure 20: Admission Rates at Other Ivy-Plus Colleges by Parental Income and Ratings at a Given Ivy-Plus College

(a) Academic Ratings, Controlling for Test Score



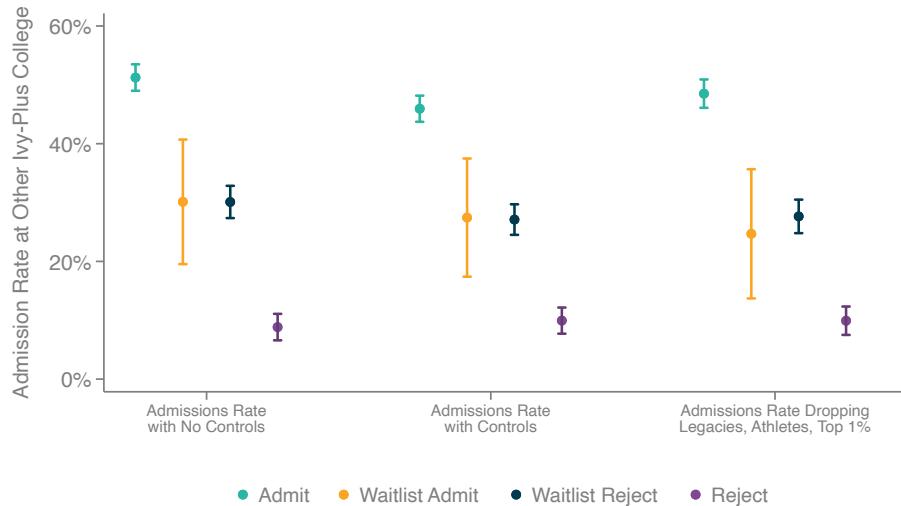
(b) Non-Academic Ratings, Controlling for Test Score



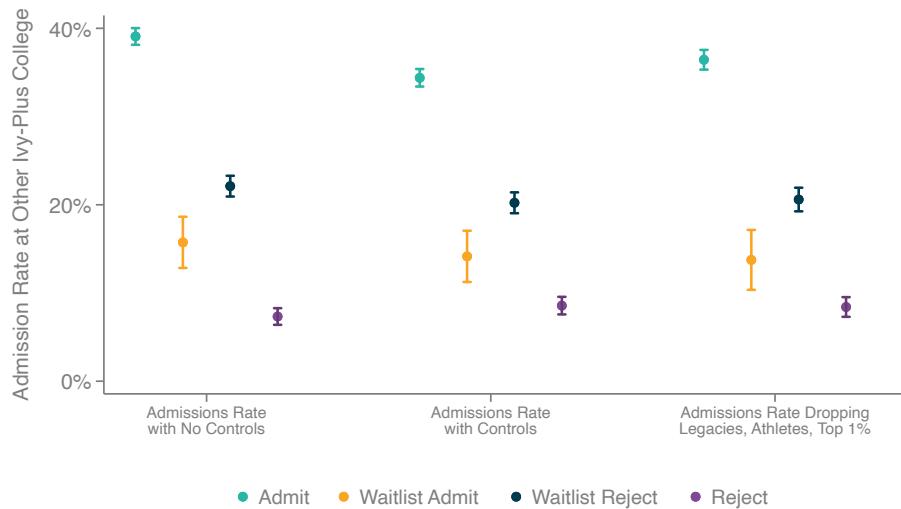
Notes: Appendix Figure 20 shows how admissions rates (reweighted by test score, as in Figure 2b) at other Ivy-Plus colleges in our college-specific sample vary with academic and non-academic ratings for applicants to the Ivy-Plus college for which we have the most granular ratings information, by parent income bin. We exclude legacies, recruited athletes, and faculty children, as well as students with missing ratings from this figure.

Appendix Figure 21: Multiple-Rater Test for Idiosyncratic Variation in Admissions: Sensitivity Analyses

(a) Excluding Private High School Attendees



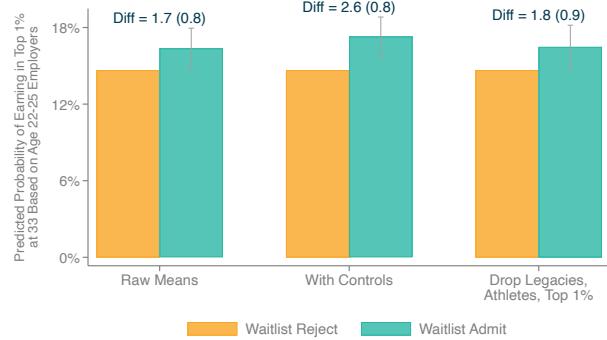
(b) Comparing Admissions Outcomes at all Ivy-Plus Colleges



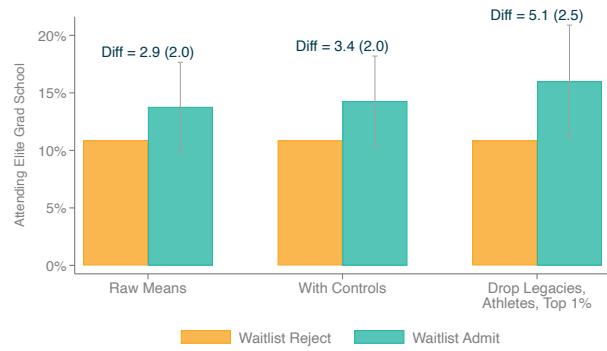
Notes: Appendix Figure 21a replicates Figure 7, excluding students from private high schools. Appendix Figure 21b replicates Figure 7 except that we consider admissions outcomes at all other Ivy-Plus colleges in our college-specific sample, not just outcomes at higher-ranked colleges. See Figure 7 for further details.

Appendix Figure 22: Treatment Effects of Ivy-Plus Admissions on Post-College Outcomes for Waitlisted Applicants in the Higher-Ranked Multiple-Rater Test Subsample

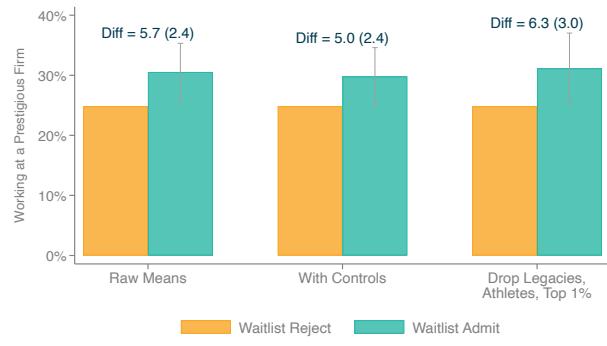
(a) Predicted Earnings in Top 1% Based on Firms at Ages 22-25



(b) Elite Graduate School Attendance



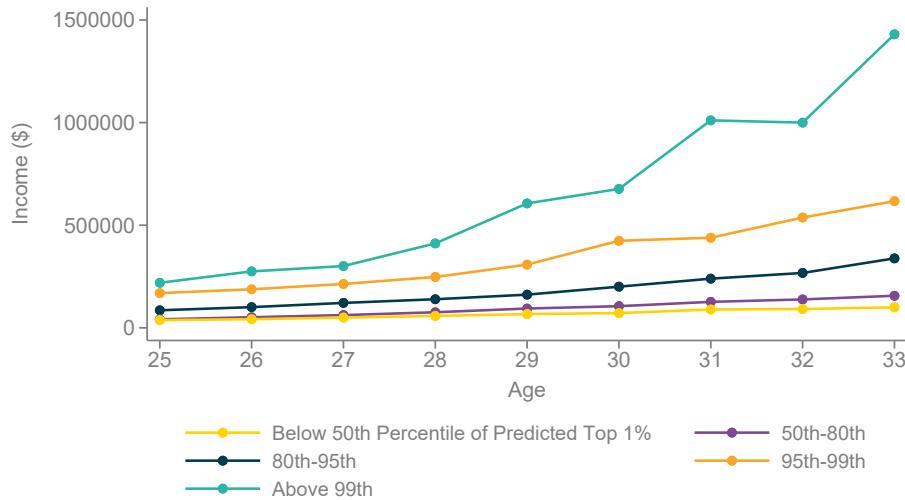
(c) Employment at a Prestigious Firm



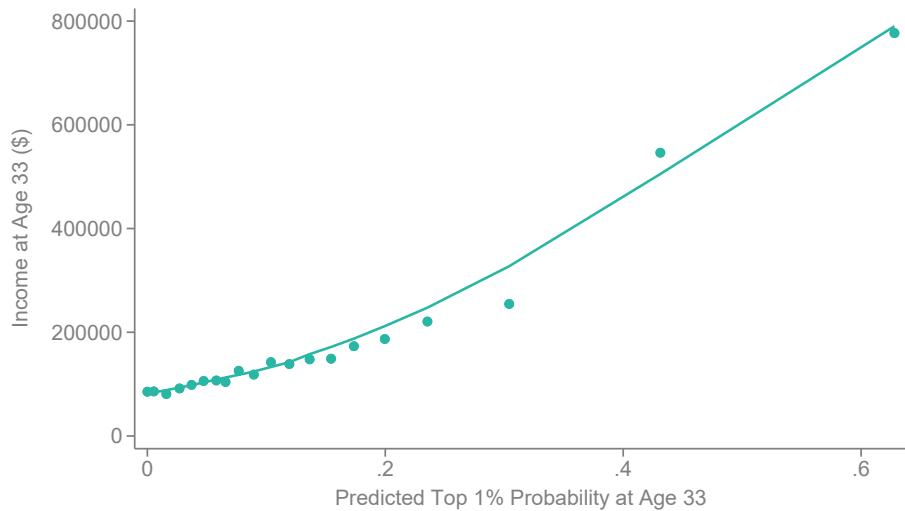
Notes: Appendix Figure 22 replicates Figures 8b, 11a, and 11c for the subset of Ivy-Plus applicants to colleges that pass at least one multiple-rater test with another (lower-ranked) Ivy-Plus college; see Section 4 for details on the test and the resulting subset of colleges.

Appendix Figure 23: Actual Incomes vs. Predicted Probabilities of Reaching Top 1%

(a) Income Trajectories by Predicted Top 1% Probability



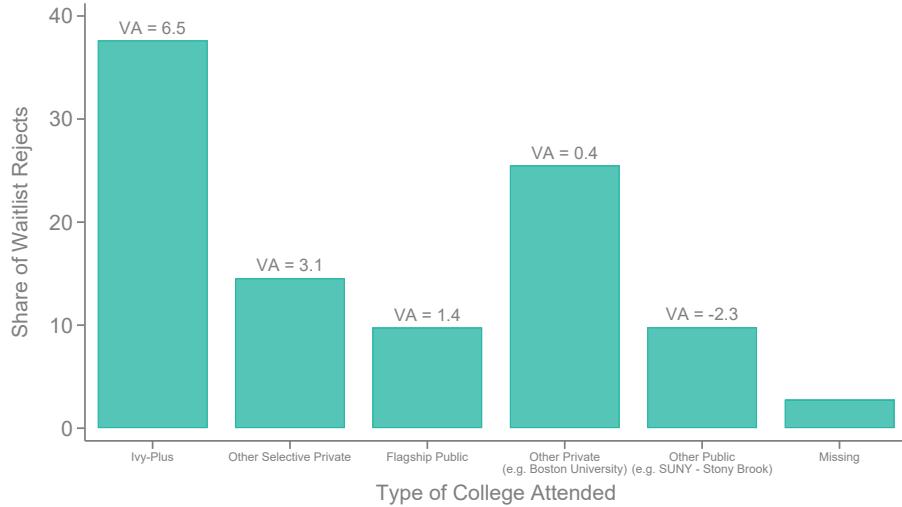
(b) Income at Age 33 vs. Predicted Top 1% Probability



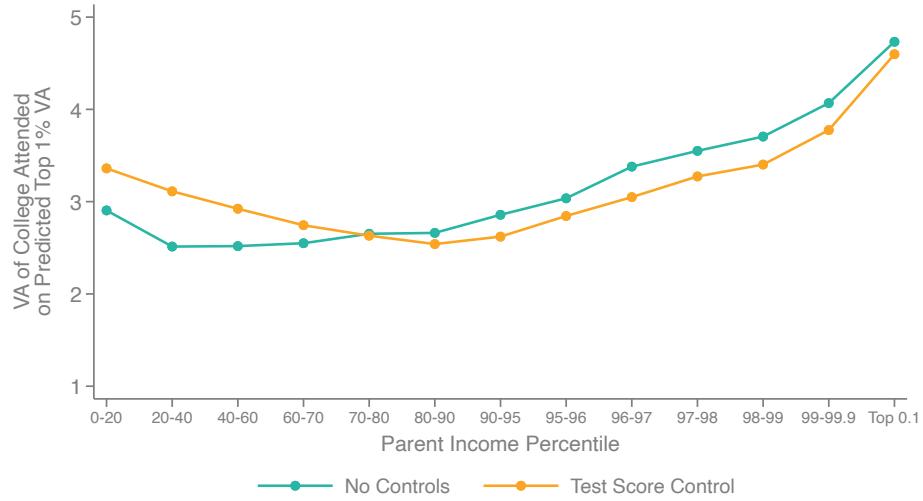
Notes: Appendix Figure 23 plots the relationship between actual incomes and predicted top 1% probability among individuals admitted to or waitlisted at Ivy-Plus colleges. In Appendix Figure 23a, we group these individuals into five bins based on their predicted top 1% probabilities. For each bin, we plot mean actual incomes (in real 2015 dollars) at each age from 25 to 33. In Appendix Figure 23b, we group individuals into 20 bins based on their predicted top 1% probabilities. All bins are equally sized except for the leftmost bin, which contains a mass point. We then plot mean actual incomes at age 33 within each bin and fit a lowess regression to the plotted points.

Appendix Figure 24: Distribution of Outside Options for Ivy-Plus Applicants Rejected from Waitlist

(a) Distribution of Colleges Attended by Rejected Applicants

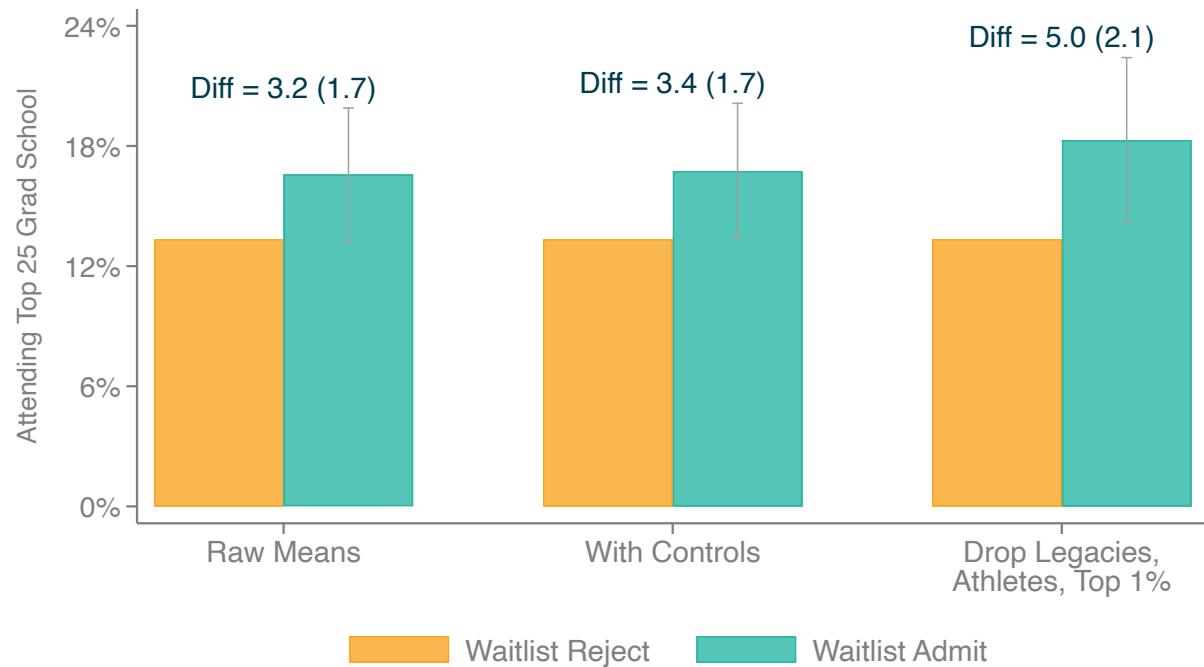


(b) Value-Added of Colleges Attended by Rejected Applicants, by Parental Income



Notes: Appendix Figure 24a shows the distribution of colleges attended by applicants to Ivy-Plus colleges in our college-specific sample who were rejected from the waitlist. We divide colleges into five groups: Ivy-Plus, Other Selective Private, and the flagship publics listed in Appendix Table 1 and then other private and other public colleges. The VA estimate listed on top of each of the bars reports the mean observational value-add (VA) estimate for colleges in each group on students' predicted probability of reaching the top 1% based on their ages 22-25 employers, estimated as described in the notes to Figure 10. Appendix Figure 24b shows how students' outside options vary across the parent income distribution. It plots the mean observational VA of the college attended by applicants rejected from the waitlist at Ivy-Plus colleges by parent income bin. The green line plots raw means (i.e., without any controls), while the orange line reweights applicants within each parent income bin to match the test score distribution of all Ivy-Plus attendees.

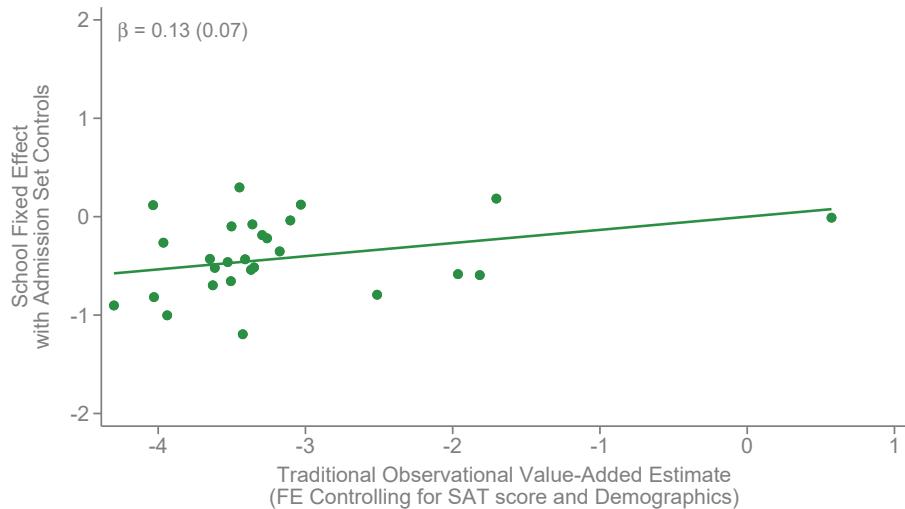
Appendix Figure 25: Impacts of Ivy-Plus College Attendance on Probability of Attending a Top-25 Graduate School



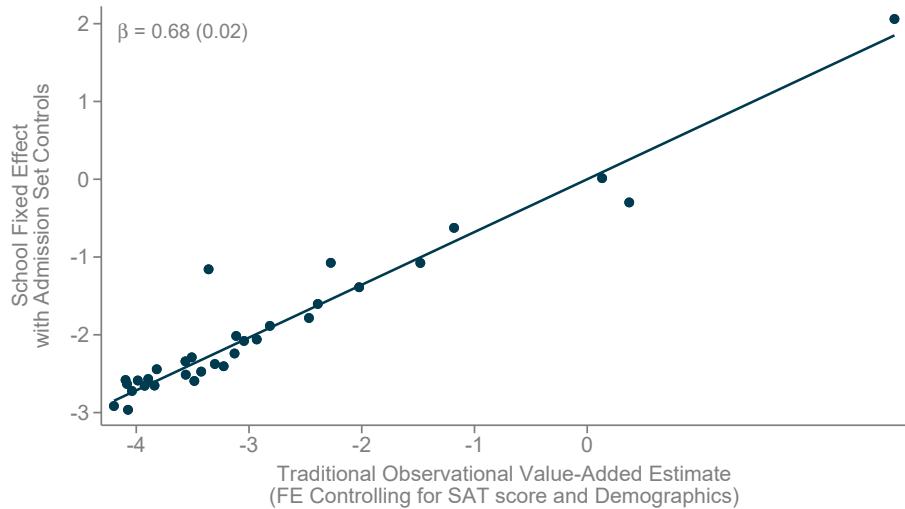
Notes: This figure replicates Figure 11a using attending a top 25 graduate schools by age 28 as the outcome. Top 25 graduate schools are defined by 2023-24 U.S. News & World Report rankings, averaging ranks across business, education, engineering, law, fine arts, and the natural and social sciences.

Appendix Figure 26: Estimates of College Effects Using Matriculation Design in Texas vs. California Public Colleges

(a) Texas

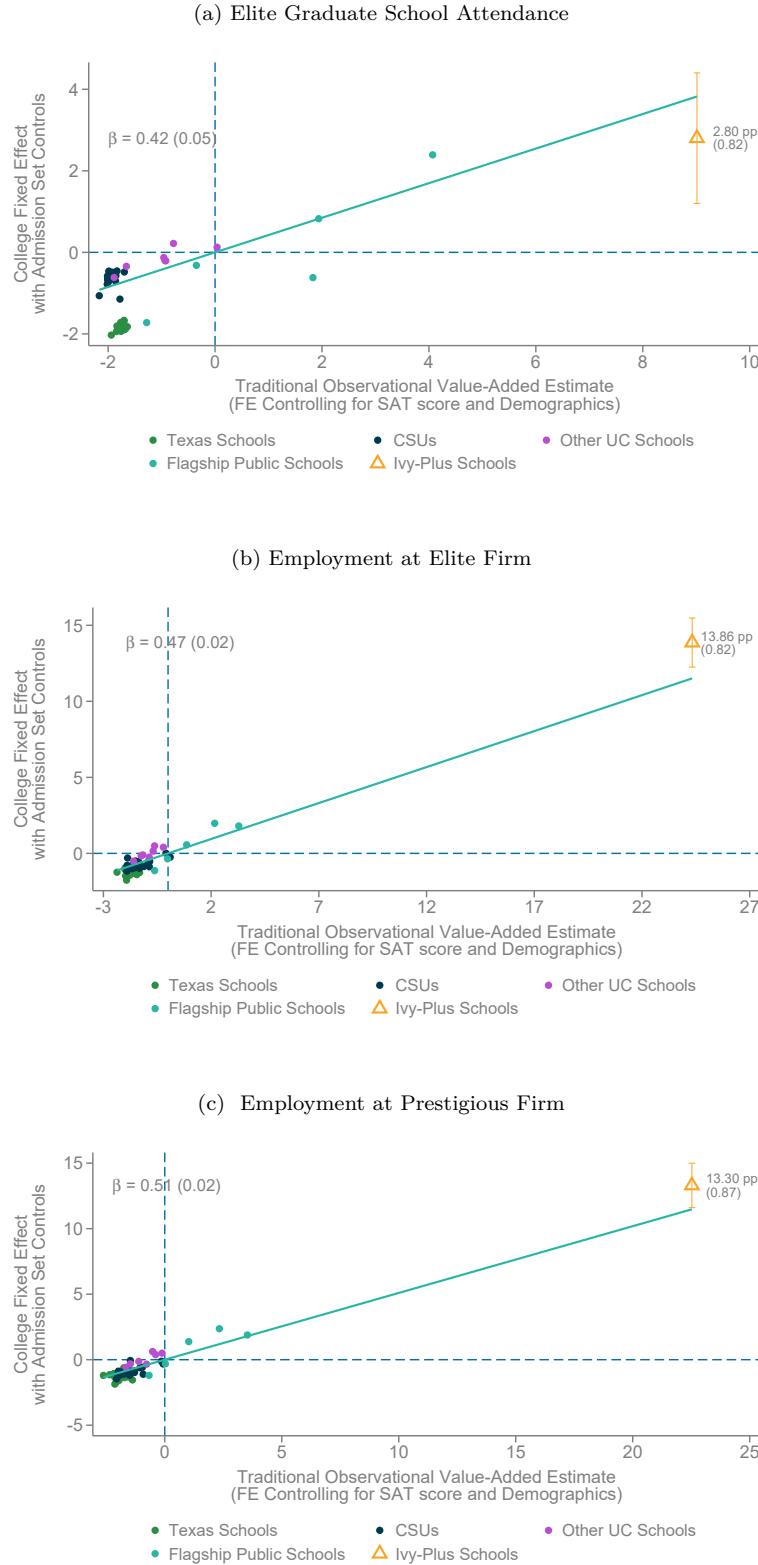


(b) California



Notes: Appendix Figure 26 replicates Figure 12b restricting to in-state applicants to public colleges in Panel A and public colleges in California in Panel B.

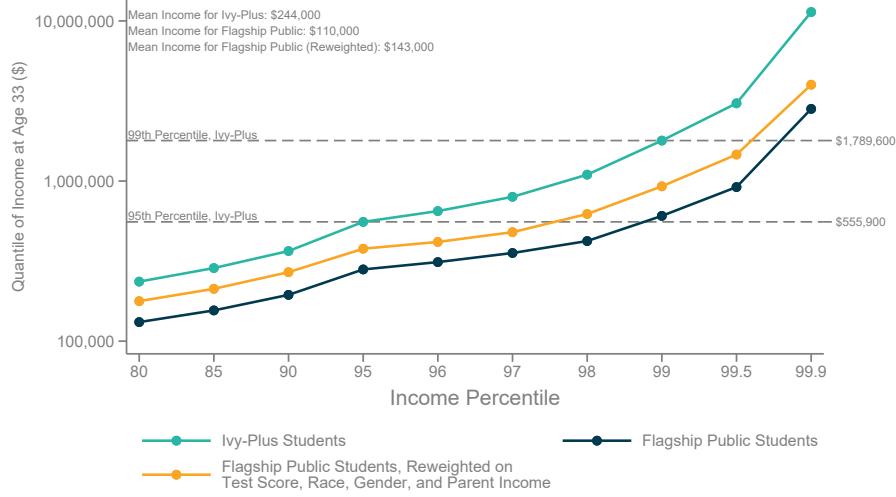
Appendix Figure 27: Causal Effects of Ivy-Plus Attendance on Non-Monetary Outcomes Based on Matriculation Design



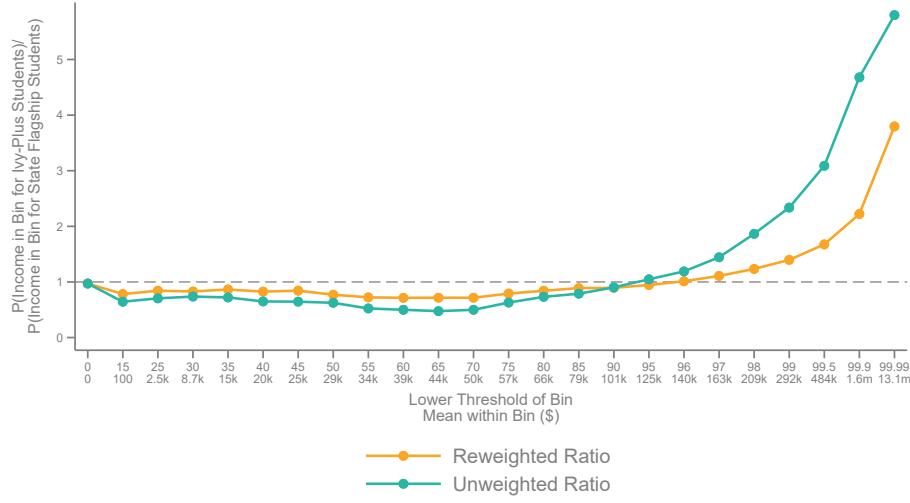
Notes: Appendix Figure 27 replicates Figure 12b using non-monetary outcomes: elite graduate school attendance at age 28 (Panel A), employment at an elite firm at age 25 (Panel B), and employment at a prestigious firm at age 25 (Panel C). See notes to Figure 11 for definitions of these outcomes.

Appendix Figure 28: Income Distributions of Ivy-Plus vs. Flagship Public Students at Age 33

(a) Quantiles of Income Distribution at Age 33 for Ivy-Plus vs. Flagship Public Students

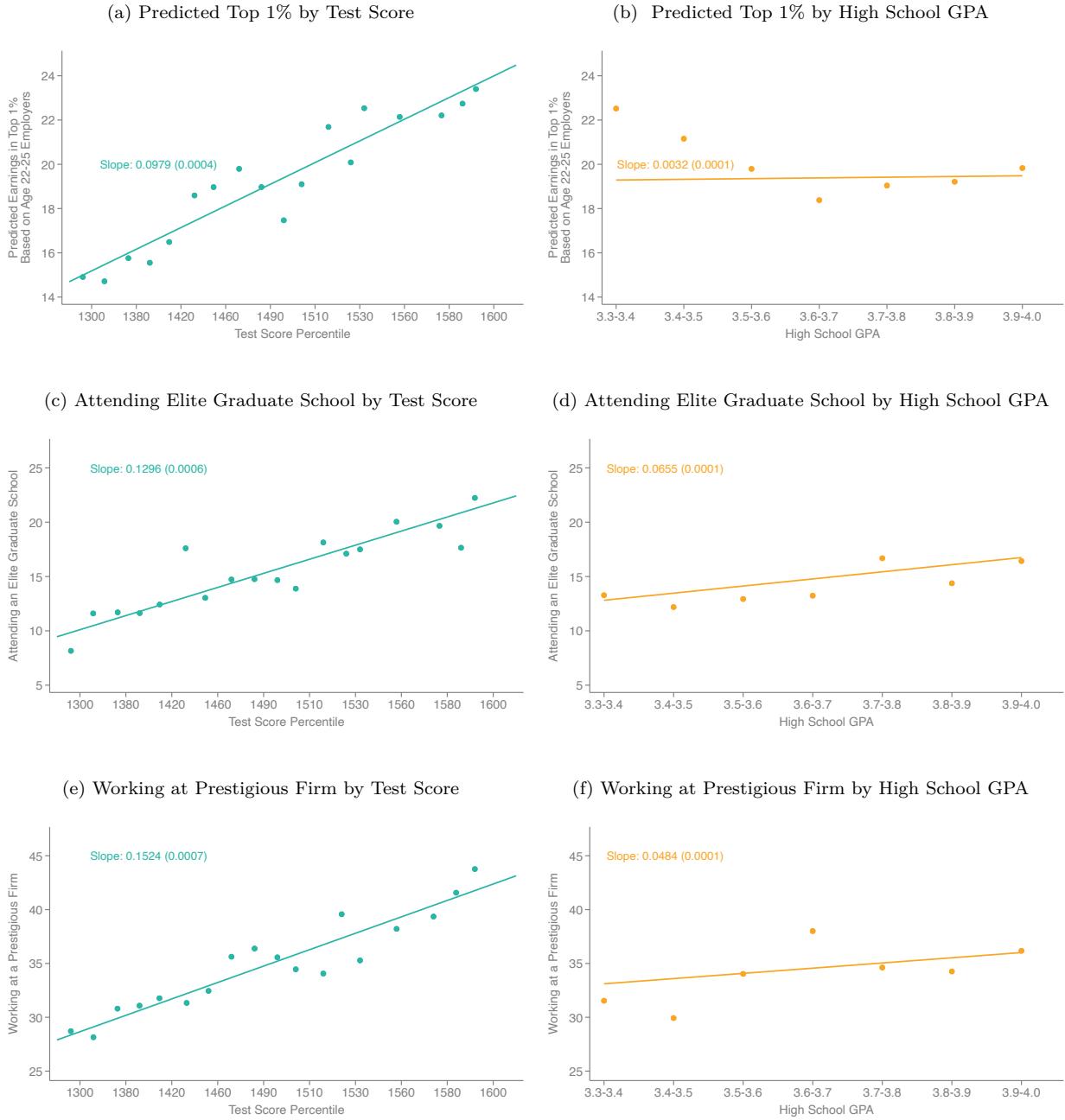


(b) Ratio of Density of Income Distribution at Age 33 for Ivy-Plus vs. Flagship Public Students



Notes: Appendix Figure 28 compares the distribution of total pre-tax individual income (as defined in Section 2) at age 33 for students who attended an Ivy-Plus vs. one of the nine flagship public colleges listed in Appendix Table 1. 28a plots quantiles of the distribution of earnings for Ivy-Plus students (green), flagship public students (dark blue), and flagship public students reweighted to match Ivy-Plus students on test score, race, gender, and parent income bin (orange), following the method in Figure 2. Appendix Figure 28b plots the ratio of the share of Ivy-Plus students in each bin of the national income distribution shown on the x axis to the corresponding share for students who attended flagship public colleges. We label the bottom percentile threshold for each bin on the x-axis. For example, the point labeled 15 on the x-axis plots the previously described ratio for individuals with earnings between the 15th and 25th percentiles of the national income distribution. Note that individuals with earnings below the 15th percentile all have zero earnings. The green line plots the raw ratio; the orange line plots the same ratio after reweighting flagship public students to match Ivy-Plus students on test score, race, gender, and parent income bin. The values listed below the x-axis labels in this figure show the mean income within each bin. The sample consists of all students in our long-term outcomes sample for whom we observe college attendance, SAT/ACT scores, and income at age 33.

Appendix Figure 29: Ivy-Plus Matriculants' Outcomes by Test Score and High School GPA



Notes: Appendix Figure 29 presents binned scatter plots of outcomes vs. ACT/SAT test scores or high school GPA for students enrolled at Ivy-Plus colleges in our college-specific sample where we have high school GPA information. Panels A, C, and E present binned scatter plots of students' post-college outcomes on test scores, controlling for parent income bin, indicators for race, gender, legacy status, recruited athlete status, and high school GPA. Panels B, D, and F repeat this but using high school GPA as the x-axis variable and controlling for the same set of variables plus test score (and without high school GPA). In these panels we plot one dot per tenth-point GPA bin, rather than by ventile. In all panels, we restrict the sample to domestic matriculants with test scores at or above an SAT of 1200 or ACT of 27 and GPA at or above 3.3; each pair of binscatters with the same outcome variable are estimated on exactly the same sample. We also report slopes based on OLS regressions (with standard errors in parentheses) estimated in the underlying microdata. Outcome measures are predicted top 1% earnings based on ages 22-25 firms (Panels A and B), attendance at elite graduate school at age 25 (Panels C and D), and working at a prestigious firm at age 25 (Panels E and F). See notes to Figure 11 for definitions of these outcomes.

Appendix Figure 30: Ivy-Plus Applicants' Outcomes by Test Score Quartile, Adjusting for Value-Added of College Attended



Notes: Appendix Figure 30 plots the relationship between students' post-college outcomes and their SAT/ACT test score quartile among waitlisted and admitted Ivy-Plus applicants, adjusting for the value-added of college attended. To construct Appendix Figure 30a, we regress the predicted probability of having earnings in the top 1% based on ages 22-25 firms on indicators for test score quartile, race, gender, and parent income bin in the bars on the left and additionally for indicators for high school GPA on the right. We then repeat this regression with the observational VA of college attended (multiplied by the ratio of the waitlist-design treatment effect estimate to the observational VA estimate reported in Columns 1 and 5 of Appendix Table 8) as the dependent variable. Each quartet of bars plots the four test-score-bin coefficients from the first regression minus those from the second. Appendix Figures 30b and 30c replicate 30a but using indicators for attending an elite graduate school at age 25 or working at a prestigious firm at age 25, respectively, as the outcome variables. Whiskers show 95 percent confidence intervals. See notes to Figure 11 for definitions of these outcomes.

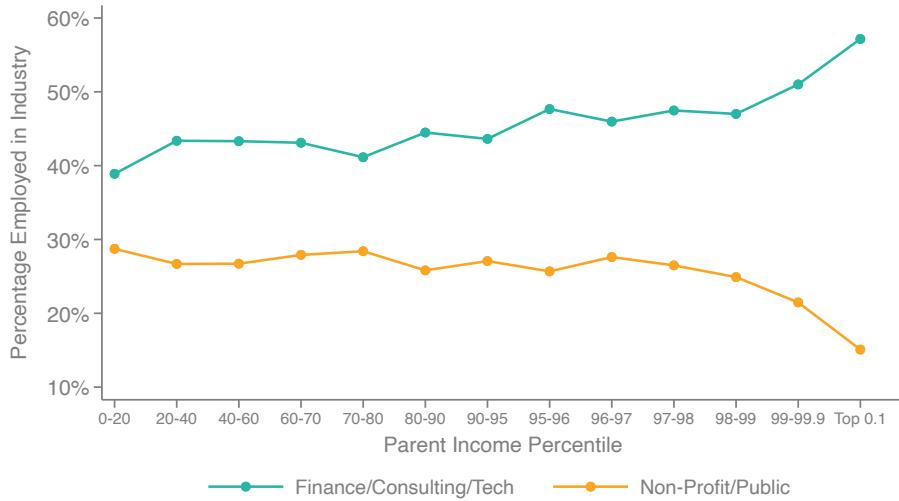
Appendix Figure 31: Share of Ivy-Plus Attendees in Top 1% of Income Distribution at Age 33 by Parental Income, Controlling for Test Scores



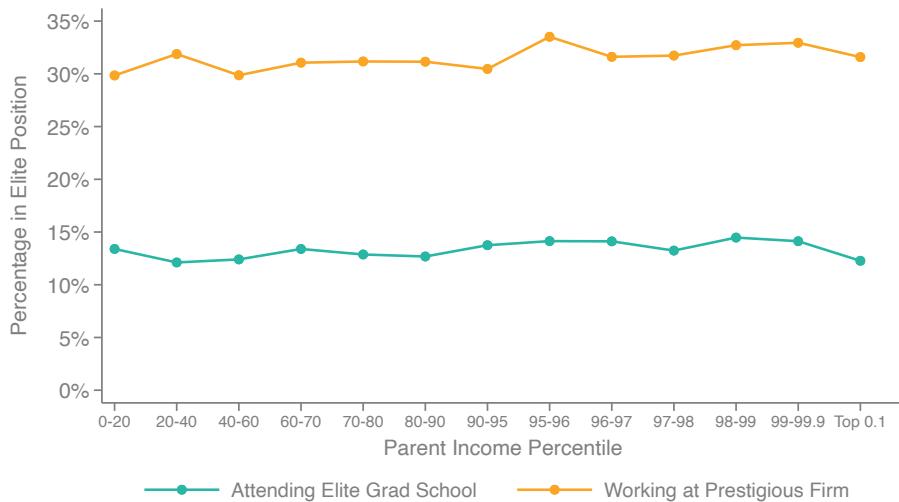
Notes: Appendix Figure 31 plots the share of Ivy-Plus students in the top 1% at age 33 based on total individual income (as defined in Section 2) or W-2 wage earnings by parent income percentile, controlling for a quintic in test scores.

Appendix Figure 32: Post-College Outcomes for Ivy-Plus Matriculants, by Parental Income

(a) Industry of Employment

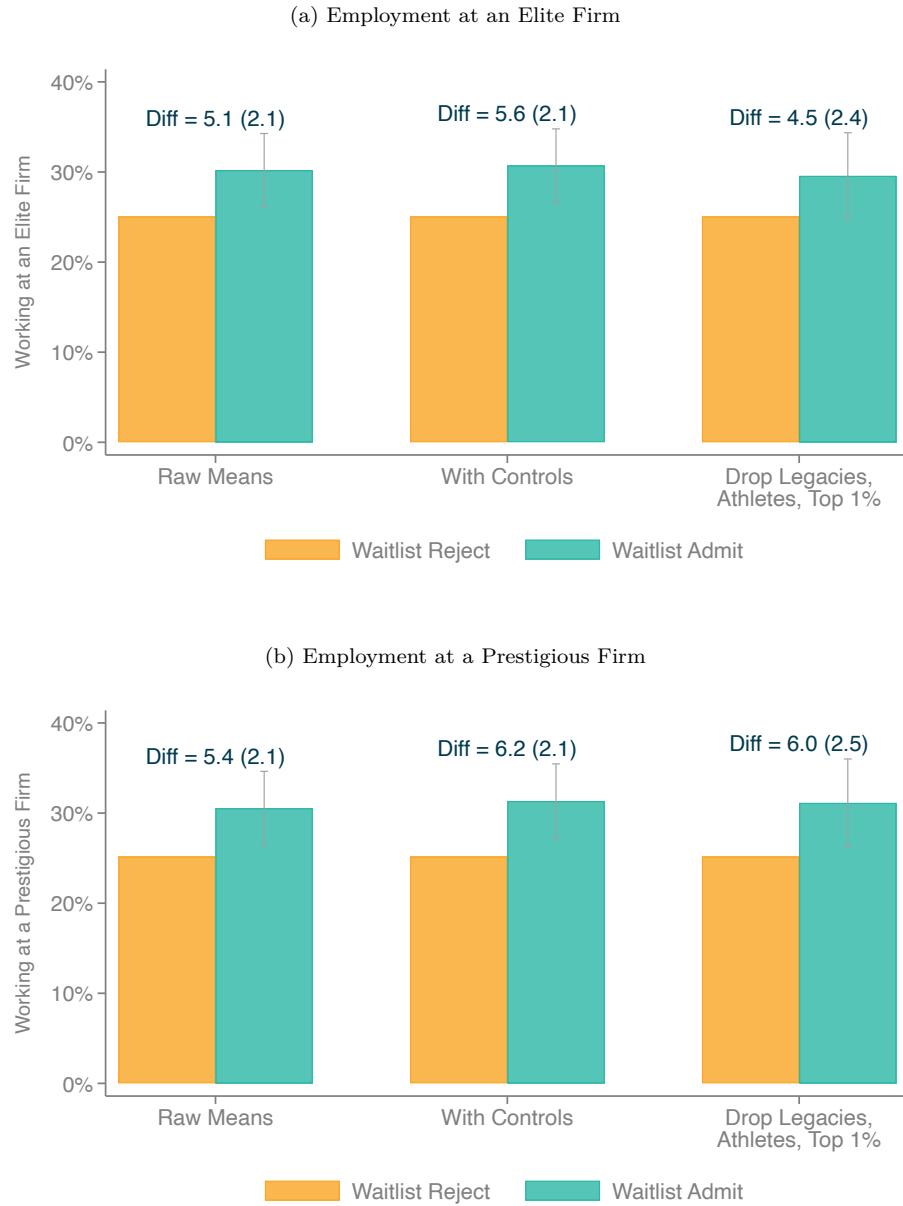


(b) Elite Graduate School Attendance and Prestigious Firm Employment



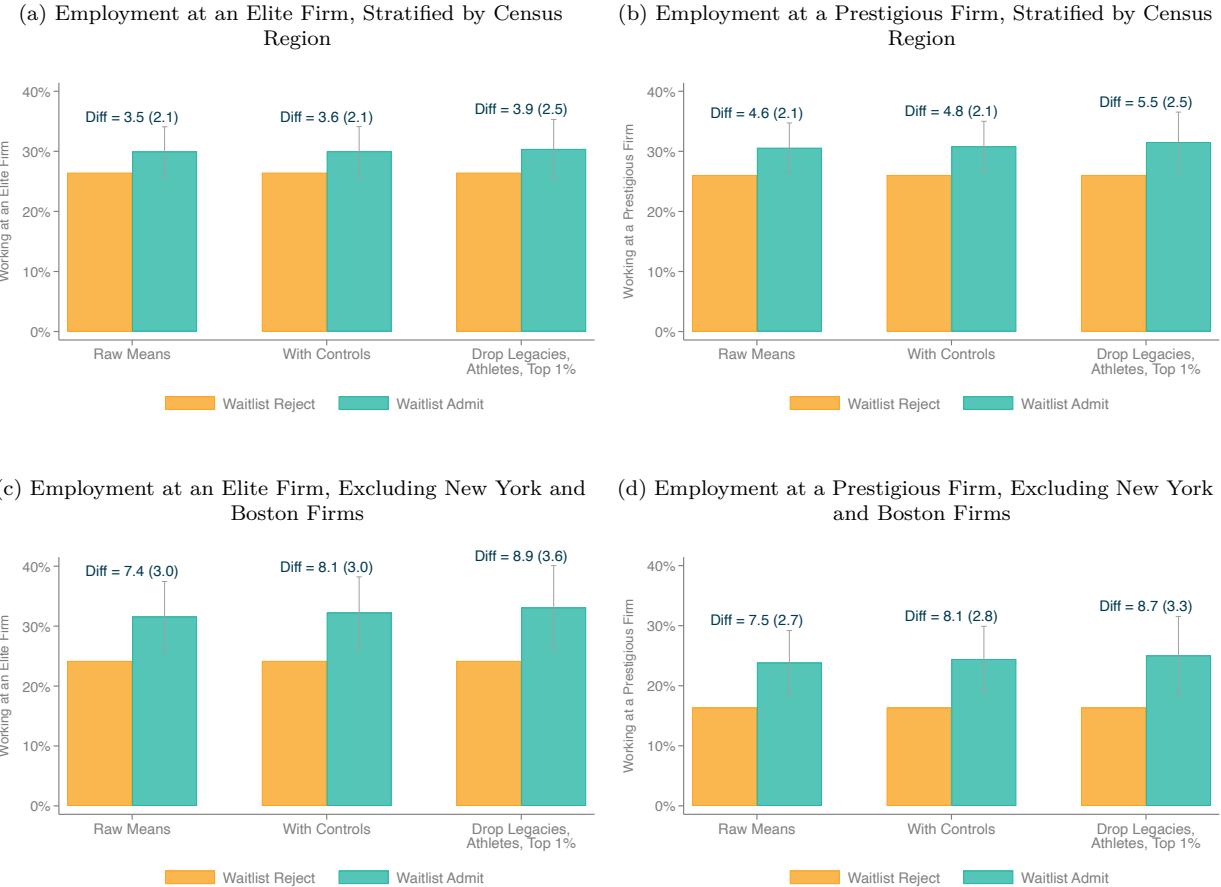
Notes: Appendix Figure 32a plots the share of Ivy-Plus matriculants working in Finance/Consulting/Tech or Non-Profit/Public industries at age 25 vs. parental income, controlling for a quintic in test scores. Industries are identified based on the employer from which individuals received the largest payment (based on W-2 forms) at age 25. Finance/Consulting/Tech includes firms with NAICS codes beginning with 51, 52 and 54. Non-Profit/Public includes the 2-digit NAICS codes 61, 62, and 92. Appendix Figure 32b plots the share of matriculants who attend an elite graduate school or work at a prestigious firm at age 25 vs. parental income, controlling for a quintic in test scores. Elite graduate schools are defined as Ivy-Plus institutions, as well as UC-Berkeley, UCLA, UCSF, University of Michigan, and University of Virginia. Elite firms are defined as firms that employ the highest share of Ivy-Plus graduates relative to graduates of flagship public colleges (leaving out the individual's own college); prestigious firms are identified based on the same ratio, controlling for the share of individuals at the firm with income in the top 1%. See Section 2 for more details on the definitions of these variables.

Appendix Figure 33: Treatment Effects of Ivy-Plus Admission on Elite and Prestigious Firm Employment Defined Using a Broader Set of Comparison Colleges



Notes: Appendix Figure 33 replicates Figures 11b and 11c, defining elite and prestigious firms as firms that employ the highest share of Ivy-Plus graduates relative to graduates of Tiers 2-4 colleges as classified by Barron's (leaving out the individual's own college).

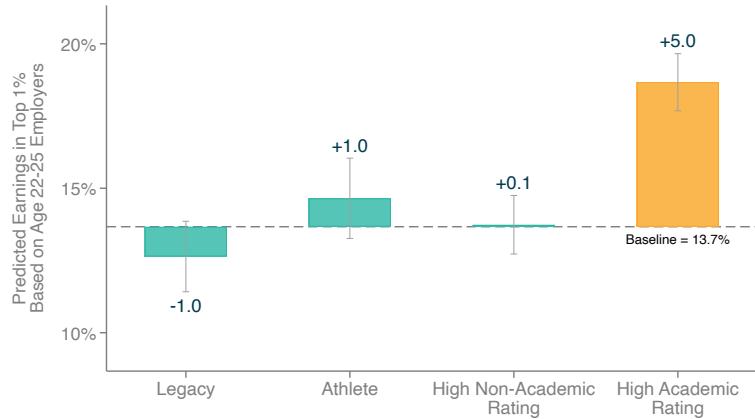
Appendix Figure 34: Treatment Effects of Ivy-Plus Admission on Elite and Prestigious Firm Employment, Controlling for Geography



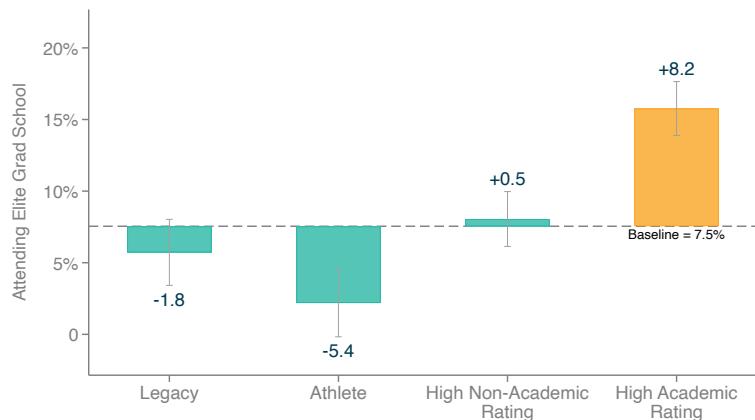
Notes: Appendix Figure 34 replicates Figures 11b and 11c, using alternative definitions for “elite” and “prestigious” firms. In Appendix Figures 34a and 34b, we rank firms exactly as in Figures 11b and 11c and then grant “elite” and “prestigious” firm status to firms until we have accounted for the top 25% of Ivy-Plus employment separately by Census Region, as opposed to the top 25% of Ivy-Plus employment nationally. In Appendix Figures 34c and 34d, we drop from the sample any individuals whose W-2 addresses are in the Boston or New York commuting zones, and then construct our elite and prestigious firm definitions exactly as in Figures 11b and 11c.

Appendix Figure 35: Association Between Post-College Outcomes and Admissions Criteria among Ivy-Plus Matriculants

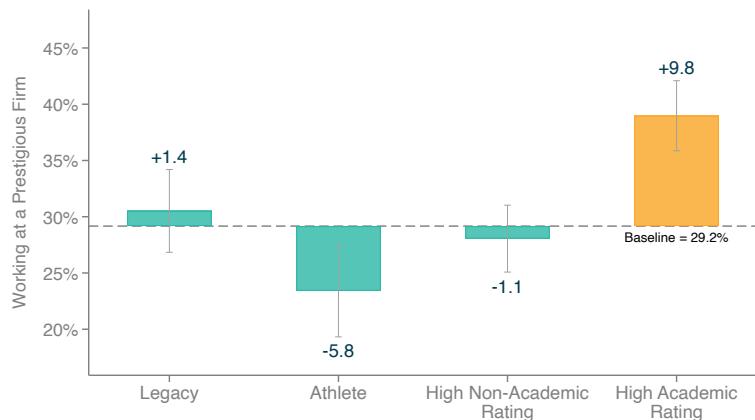
(a) Association Between Predicted Top 1% Earnings and Admissions Criteria



(b) Association Between Elite Graduate School Attendance and Admissions Criteria

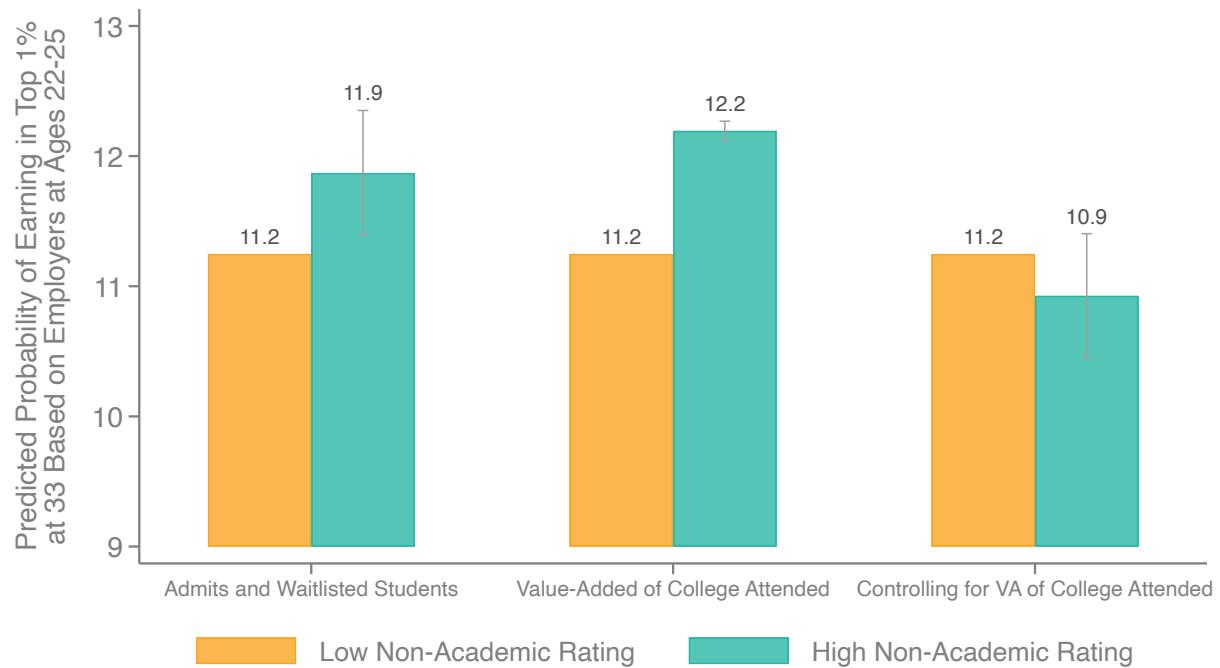


(c) Association Between Prestigious Firm Employment and Admissions Criteria



Notes: Appendix Figure 35 replicates the “Raw Comparison” estimates from Figure 14a, except restricting to the students who attended the Ivy-Plus college with the most granular ratings information (i.e., focusing on attendees, not all applicants). Panels B and C replicate Panel A using other post-college outcomes.

Appendix Figure 36: Post-College Outcomes Among Ivy-Plus Applicants by Non-Academic Ratings



Notes: Appendix Figure 36 compares the predicted top 1% share based on firms at ages 22-25 of applicants who receive high vs. low non-academic ratings. The sample consists of applicants who were either admitted or offered a place on the waitlist from the Ivy-Plus college with the most granular ratings information in our college-specific sample. The first, third, and fifth bars show the mean predicted top 1% share of applicants with a low non-academic rating. The second bar adds the coefficient on high non-academic rating to the first bar, estimated using a regression of predicted top 1% on an indicator for high non-academic rating, high academic ratings, legacy status, and being a recruited athlete. The fourth bar repeats the second bar, except using the observational value-added of the college the student attended (multiplied by the ratio of the waitlist-design treatment effect estimate to the observational VA estimate reported in Columns 1 and 5 of Appendix Table 8) as the dependent variable in the regression. The last bar adds the difference between the 2nd and 4th bars to the level in the 5th bar to obtain the difference in outcomes by non-academic rating adjusted for the value-added of the college the student attends.